TA
401
A5
Vol. 2
1902

# AMERICAN SOCIETY
FOR
# TESTING MATERIALS.

AFFILIATED WITH THE
INTERNATIONAL SOCIETY FOR TESTING MATERIALS.

## PROCEEDINGS

OF THE

# FIFTH ANNUAL MEETING,

Held in Atlantic City, New Jersey,
June 12, 13, 14, 1902.

VOLUME II.

EDITED BY THE SECRETARY, UNDER THE DIRECTION OF
THE COMMITTEE ON PUBLICATIONS.

OFFICE OF THE SECRETARY, UNIVERSITY OF PENNSYLVANIA, PHILADELPHIA, PA.:
PUBLISHED BY THE SOCIETY
1902.

28 Aug 69

43863

Entered according to Act of Congress, by the
AMERICAN SOCIETY FOR TESTING MATERIALS,
in the office of the Librarian of Congress, at Washington.

The Society is not responsible, as a body, for the statements and opinions advanced in its publications.

# CONTENTS.

## PROCEEDINGS.


PAGE

Summary of Proceedings of the Fifth Annual Meeting ..... 3
Annual Address by the Retiring President, Henry M. Howe ..... 17
Proposed Modifications of the Standard Specifications for Steel Rails —Topical Discussion ..... 23
Is it Desirable to Specify a Single Grade of Structural Steel for Bridges of Ordinary Spans?—Topical Discussion
Formal Discussion—A. P. Boller, T. L. Condron, Theodore Cooper, J. E. Greiner, John McLeod, C. C. Schneider, J. P. Snow ..... 50
General Discussion ..... 69
Rail Temperatures—Simon Strock Martin ..... 75
Finishing Temperature and Structure of Steel Rails—Albert Sauveur 79
Discussion of the Two Preceding Papers ..... 85
The Relation Between the Basic Open-Hearth Process and the Physical Properties of Steel—Topical Discussion ..... 97
Steel Rivets—Gaetano Lanza ..... 110
The Ethics of Testing—Paul Kreuzpointner ..... 118
Standard Cement Specifications—R. W. Lesley ..... 121
The Advantages of Uniformity in Specifications for Cement and Methods of Testing—George S. Webster ..... 128
Discussion of the Two Preceding Papers ..... 133
The Chemical Analysis of Cement: Its Possibilities and Limitations—Richard K. Meade ..... 139
Discussion ..... 146
Cement Testing in Municipal Laboratories—Richard L. Humphrey .. 150
Tests of Reinforced Concrete Beams—W. Kendrick Hatt ..... 161
Effect of Variations in the Constituents of Cast Iron—W. G. Scott .. 181
Discussion ..... 200
Present Status of Testing Cast Iron—Richard Moldenke ..... 207
The Need of Foundry Experience for the Proper Inspection and Testing of Cast Iron—Thos. D. West ..... 210
Discussion of the Two Preceding Papers ..... 213
A Quick and Automatic Taper Scale Test—Asa W. Whitney ..... 217
High Strength of White Iron Castings as Influenced by Heat Treatment—Alex. E. Outerbridge, Jr. ..... 229
Discussion ..... 240
Notes on Current Specifications for Cast Iron Pipe—Walter Wood ... 243
On the Constitution of Cast Iron—Henry M. Howe ..... 246
Discussion ..... 276

## APPENDICES.

PAGE

Appendix I. Report on the Buda-Pesth Congress—Henry M. Howe ..... 279
Appendix II. Bibliography on Impact Tests and Impact Testing Machines—W. Kendrick Hatt and Edgar Marburg ..... 283
Appendix III. Rules for Standard Tests of Materials Formulated by the German Association for Testing Materials (English Translation) ..... 298

## CHARTER, BY-LAWS, LIST OF MEMBERS, ETC.

Charter of the American Society for Testing Materials ..... 320
By-Laws of the American Society for Testing Materials ..... 322
Rules Governing the Executive Committee ..... 325
General Information Concerning:
(*a*) The International Association for Testing Materials ..... 326
(*b*) The Organization of the American Members ..... 328
Officers, Members of the Executive Committee and Standing Committees ..... 331
List of Members of the American Society for Testing Materials ..... 332
Geographical Distribution of Members ..... 356
Deceased Members ..... 356
Past Officers ..... 357
Annual Report of the Executive Committee ..... 358
Appendix A. Abstract of Minutes of the Executive Committee ..... 360
Appendix B. Charter and Proposed By-Laws ..... 362
Report of Auditing Committee ..... 365
Previous Publications, Contents of ..... 366
The International Association for Testing Materials.
Officers ..... 369
By-Laws ..... 370
Technical Problems, Committees and Referees ..... 374
Technolexicon ..... 381
Subject Index ..... 383
Author Index ..... 386

## PLATES.

I. Micro-photographs of Steel Rails ..... 78
II. Tables of Estimated Ultimate Strength of Steel ..... 104
III. Tables of Estimated Ultimate Strength of Steel ..... 104
IV. Micro-photographs of Cast Iron ..... 224
V. Physical Properties and Assumed Microscopic Constitution of Cast Iron ..... 252
VI. The Relation between Tensile Strength of Cast Iron and the Condition of Carbon ..... 266

## SUMMARY OF PROCEEDINGS OF THE FIFTH ANNUAL MEETING.

### Atlantic City, N. J., June 12, 13, 14, 1902.

---

The Fifth Annual Meeting of the American Section of the International Association for Testing Materials (now the American Society for Testing Materials) was held at the Hotel Traymore, Atlantic City, N. J., on June 12, 13, 14, 1902. The total attendance at the meeting, including guests, was about seventy-five.

The following members were present or were represented at the meeting: W. A. Aiken, Ajax Metal Company, represented by G. H. Clamer; American Bridge Company, represented by C. C. Schneider; American Steel and Wire Company, represented by William L. Hirsch; Joseph W. Bramwell, W. H. Broadhurst, H. H. Campbell (also representing The Pennsylvania Steel Company), Carnegie Steel Company, represented by John McLeod; Robert A. Carter, Charles S. Churchill, James Christie, W. C. De Armond, J. Allen Colby, T. L. Condron, W. O. Dunbar, W. C. Du Comb, Jr., Charles B. Dudley, M. Ward Easby, Ernst Fahrig, A. Falkenau, Stanley G. Flagg, Jr., J. E. Greiner, Edward M. Hagar, William K. Hatt, Henry M. Howe, Richard L. Humphrey, Charles L. Huston (also representing the Lukens Iron and Steel Company), Illinois Steel Company, represented by P. E. Carhart, J. Y. Jewett, Robert Job, C. Kirchoff, Paul Kreuzpointner, Gaetano Lanza, Lathbury & Spackman, represented by E. W. Lazell; Robert W. Lesley, Charles Major, Edgar Marburg, Richard K. Meade, Charles M. Mills, Richard G. G. Moldenke (representing also the American Foundrymen's Association), Tinius Olsen, Alex. E. Outerbridge, Jr., Clifford Richardson, Albert Sauveur, H. W. Spangler, William Purves Taylor, George E. Thackray (representing also the Cambria Steel Company), David Thomas (representing also the Reading Iron Company), *The Iron Trade Review*, represented by A. I. Findley; S. S. Voorhees, Samuel Tobias Wagner, George S. Webster, William R. Webster, Thomas D. West, J. W. Whitehead, Jr., Asa W.

Whitney, Walter Wood (representing also the R. D. Wood Company). Total, 63 (including representations); total in personal attendance, 57.

---

## FIRST SESSION, THURSDAY, JUNE 12, 8 P. M.

### BUSINESS MEETING.

The meeting was called to order by the Chairman, Professor Henry M. Howe. There being no objection, the minutes of the Fourth Annual Meeting were approved as printed.

Then followed the reading of the Annual Address by the Chairman, devoted largely to a review of the activities of the past year and an outline of a general policy for the future.

The Annual Report of the Executive Committee was read by the Secretary and, on motion, adopted.

The Chair appointed Messrs. George E. Thackray and Richard L. Humphrey as tellers to canvass the ballot for officers.

The meeting then proceeded to the consideration of the new Charter and the proposed By-Laws under the same. Mr. R. W. Lesley announced that the incorporators had duly met, that Art. I, Sections 1 and 2, of the By-Laws had been formally adopted, and that the Secretary had, according to instructions, cast a ballot electing all members of the American Section of the International Association for Testing Materials to membership in the American Society for Testing Materials.

After a short recess the tellers reported that 52 ballots, of which 50 were in legal form, had been cast, and in accordance with the report, the Chair declared the following officers elected:

Chairman, Charles B. Dudley.
Vice-Chairman, Robert W. Lesley.
Secretary, Edgar Marburg.
Treasurer, James Christie.
Member of Executive Committee, John McLeod.

The proposed By-Laws were then considered by sections and adopted with certain amendments; * the most important being the addition of the following sections to Art. II:

* These amendments will appear from a comparison of the By-Laws as adopted (page 322), and as reported by the Executive Committee (see Annual Report, Appendix B, page 363).

"SECTION 7. The officers and members of the Executive Committee of this Society, to hold office until the next election under these By-Laws, shall be as follows: To hold office for two years—President, Charles B. Dudley; Vice-President, Robert W. Lesley; Secretary-Treasurer, Edgar Marburg; members of the Executive Committee, Henry M. Howe and James Christie. To hold office for one year—members of the Executive Committee, Albert Ladd Colby and John McLeod."

"SECTION 8. The above officers and members of the Executive Committee, as well as all succeeding officers and members of the Executive Committee elected under these By-Laws, shall serve for the respective terms to which they shall have been elected, or until their successors shall have been duly elected."

"SECTION 9. The Executive Committee shall have the power to fill any vacancies occurring in their number, by death, resignation or otherwise."

"SECTION 10. The election of officers and members of the Executive Committee shall be by letter ballot. The Executive Committee, before each Annual Meeting, shall appoint a Nominating Committee, whose duty it shall be to nominate a full list of officers. The list of nominations so made shall be submitted to the membership not more than eight (8) nor less than four (4) weeks before the coming Annual Meeting.

"Further nominations, signed by at least ten (10) members, may be submitted in writing to the Secretary, at least four (4) weeks before the Annual Meeting, and such nominations shall also be submitted to the membership on the official ballot."

The By-Laws in their amended form were finally adopted as a whole, and the Secretary was instructed to cast a letter ballot for the officers named in Art. II, Section 7.

The Chair called upon the members of the American Section present to signify their acceptance of their election to membership in the American Society for Testing Materials by rising in their places. The response to this invitation was unanimous.

Mr. James Christie formally resigned the office of Treasurer of the American Section, and his resignation was accepted.

It was resolved that all assets and liabilities of the

American Section be assumed by the American Society for Testing Materials, and all committees of the former body be continued in the latter.

There being no further business, the meeting adjourned till the following morning.

---

## SECOND SESSION, FRIDAY, JUNE 13, 10 A. M.

### ON SPECIFICATIONS.

The meeting was called to order by the newly elected President, Dr. Charles B. Dudley, who called for the report of the American Branch of Committee No. 1. Its Chairman, Mr. William R. Webster, read the following report:

### REPORT OF THE AMERICAN BRANCH OF COMMITTEE NO. 1.

All of our specifications were adopted at the Annual Meeting last year, and this action was confirmed in August by letter ballot of the American Section. Since then very little has been done by this Committee on specifications; but other societies have been quite active.

The American Railway Master Mechanics' Association, at their Annual Convention, last June, appointed a Committee on Axle Specifications. They are now considering an increase in membership in the Committee so as to take up specifications for Steel Forgings, Boiler Plate, etc.

The American Society of Mechanical Engineers at their Semi-Annual Meeting in May, discussed our Specifications for Steel Forgings, Steel Castings and Steel Boiler Plate.

The American Institute of Mining Engineers, at their Annual Meeting in May, discussed our Specifications for Steel Forgings and Steel Castings. They also had a general discussion on the present situation as to specifications for Steel Rails.

The American Society of Civil Engineers have recently appointed a Special Committee on Rail Sections, with instructions to report on the following:

1. To report upon the results obtained in the use of rails of the sections presented to the Society in Annual Convention, August, 1893, by a special committee appointed for that purpose.
2. To report whether any modification of any sections is advisable, and if so, to recommend such modification.
3. To report upon the recognized practice as to chemical composition and mechanical treatment used in the manufacture of rails and the manner of inspection of same.

4. To report upon the advisability of the establishment of a form of specification covering the manufacture and inspection of rails.
5. If found advisable to recommend a form of specification for the manufacture and inspection of rails.

The American Railway Engineering and Maintenance of Way Association, at their Annual Meeting in March, continued the discussion of our Specifications for Bridge Materials and adopted, with some modifications and additions, our Specifications for Rails. Attached hereto is a copy of the report of their Committee No. 15 on Iron and Steel Structures, and Committee No. 4, on Rails. But as the points raised have been given in detail in the program of this meeting, and are to be discussed this morning, I will not refer to them further at this time.

Dr. R. G. Moldenke asked and received permission to present our Specifications for Steel Castings to the American Foundrymen's Association at their Annual Meeting in Boston, next week.

This report was, on motion, accepted.

This was followed by a prolonged general discussion on the following modifications (shown in italics) in the Standard Specifications for Steel Rails (adopted by the American Section) embodied in the Specifications for Steel Rails adopted by the American Railway Engineering and Maintenance of Way Association, at the Annual Meeting in Chicago, March 18, 19, 20, 1902.

(*a*) DROP TEST.—One drop test shall be made on a piece of rail not more than six feet long from *every* blow of steel. The test piece shall preferably be taken from the top of the ingot. The rail shall be placed head upwards on the supports, and the various sections shall be subjected to the following impact tests:

| Weight of rail. Pounds per yard. | | | Height of drop. Feet. |
|---|---|---|---|
| 45 | to and including | 55 | 15 |
| More than 55 | " | 65 | 16 |
| " 65 | " | 75 | 17 |
| " 75 | " | 85 | 18 |
| " 85 | " | 100 | 19 |

If any rail break when subjected to the drop test, two additional tests will be made of other rails from the same blow of steel, and if either of these latter tests fail, all the rails of the blow which they represent will be rejected, but if both of these additional test pieces meet the requirements, all the rails of the blow which they represent will be accepted.

(*b*) HEAT TREATMENT.—*The number of passes and speed of train shall be so regulated that on leaving the rolls at the final pass the temperature of the rail will not exceed that which requires a shrinkage allowance at the hot saws of — inches for 85-pound and — inches for 100-pound rails, and*

*no artificial means of cooling the rails shall be used between the finishing pass and the hot saws.*

(*c*) LENGTH OF RAILS.—The standard length of rails shall be *thirty-three* feet. Ten per cent (10%) of the entire order will be accepted in shorter lengths, varying by even feet down to *twenty-seven* feet (27′). A variation of one-fourth of an inch (¼″) in length from that specified will be allowed.

The corresponding clauses in the Standard Specifications, adopted by the American Section are as follows (all differences are shown in *italics*):

(*a*) DROP TEST.—One drop test shall be made on a piece of rail not more than six feet long, selected from every *fifth* blow of steel. The rail shall be placed head upwards on the supports and the various sections shall be subjected to the following impact tests:

| Weight of rail. Pounds per yard. | | | Height of drop. Feet. |
|---|---|---|---|
| 45 | to and including | 55 | 15 |
| More than 55 | " | 65 | 16 |
| " 65 | " | 75 | 17 |
| " 75 | " | 85 | 18 |
| " 85 | " | 100 | 19 |

If any rail break when subjected to the drop test, two additional tests will be made of other rails from the same blow of steel, and if either of these latter tests fail, all the rails of the blow which they represent will be rejected, but if both of these additional test pieces meet the requirements, all the rails of the blow which they represent will be accepted. *If the rails from the tested blow shall be rejected for failure to meet the requirements of the drop test as above specified, two other rails will be subjected to the same tests, one from the blow next preceding, and one from the blow next succeeding the rejected blow. In case the first test taken from the preceding or succeeding blow shall fail, two additional tests shall be taken from the same blow of steel, the acceptance or rejection of which shall also be determined as specified above, and if the rails of the preceding or succeeding blow shall be rejected, similar tests may be taken from the previous or following blows, as the case may be, until the entire group of five blows is tested, if necessary.*

*The acceptance or rejection of all the rails from any blow will depend upon the results of the tests thereof.*

(*b*) HEAT TREATMENT.—*No test specified.*

(*c*) LENGTH OF RAILS.—The standard length of rails shall be *thirty* feet (30′). Ten per cent (10%) of the entire order will be accepted in shorter lengths, varying by even feet down to *twenty-four* feet (24′). A variation of one-fourth of an inch (¼″) in length from that specified will be allowed.

Mr. William R. Webster introduced the discussion with the statement that these matters had been brought to the notice of

the American Branch of Committee No. 1 at a recent meeting, and that it was there decided to present them at the Annual Meeting so that they might be openly discussed on their merits.

The three sub-divisions of the subject were separately considered and the following resolutions were passed:

(*a*) DROP TEST.—That the matter be referred back to the American Branch of Committee No. 1, with the statement that it is the sense of this meeting that one drop test from every fifth blow of steel is sufficient.

(*b*) HEAT TREATMENT.—That action be postponed pending the reading at the afternoon session, of papers on closely related subjects by Messrs. Sauveur and Martin, and the discussion of these papers.

(*c*) LENGTH OF RAILS.—That this question be referred back to the American Branch of Committee No. 1.

The meeting then passed to the consideration of the next number on the program, viz:

Topical Discussion: "Is it Desirable to Specify a Single Grade of Open Hearth Structural Steel for Bridges of Ordinary Spans?" This discussion was formally introduced by contributions from Messrs. A. P. Boller, Theodore Cooper, J. E. Greiner, John McLeod, C. C. Schneider and J. P. Snow, and then thrown open to the meeting. The discussions of Messrs. Boller and Cooper, in the absence of the authors, were read by the Secretary. At the conclusion of the discussion, it was resolved to refer this question to Committee No. 1, with the statement that it is the sense of the meeting that it should be affirmatively indorsed.

The meeting then adjourned till 3 P. M.

---

## THIRD SESSION, FRIDAY, JUNE 13, 3 P. M.

### ON STEEL.

The President, Dr. Charles B. Dudley, after calling the assembly to order, announced that before proceeding with the formal program he would ask Professor Howe to favor the meeting with some informal remarks on the heat treatment and finishing temperatures of steel, in the light of some experimental work which he had planned in that connection.

Professor Howe explained the proposed installation of a small roll-train to be driven by a motor. The first experiment in view is to heat similar bars of steel to a uniform high temperature; to allow these bars to cool to lower temperatures of various accurately determined magnitudes; then to roll them quickly, and finally to ascertain their physical properties and microscopic structure, as a definite starting point towards a better quantitative knowledge of the subject.

Mr. Robert A. Carter asked the indulgence of the meeting and called attention to the great need of adequate specifications and methods of testing for chain-iron. The speaker stated that he had recently prepared specifications for high-grade wrought iron and thought that the question was one on which the Society might well take suitable action.

On motion, it was decided to refer this question to the American Branch of Committee No. 1, and the regular program was then resumed.

Professor Albert Sauveur read a paper on "Finishing Temperature and Structure of Steel Rails."

A paper by Mr. Simon S. Martin on "Rail Temperatures" was read by the Secretary.

A formal discussion of these papers by Mr. Robert Job was read by the author, and in the absence of Mr. P. H. Dudley, his contribution on the subject was read by the Secretary.

The Chairman, in declaring these papers open for general discussion, observed that the discussion of "Heat Treatment" at the morning session might be appropriately brought to a close at the same time.

After a prolonged discussion it was resolved that the question of introducing a clause with reference to heat treatment in the Standard Specifications for Steel Rails, be referred back to the American Branch of Committee No. 1 with the statement that it is the sense of this meeting that it is inadvisable at the present time to introduce such a clause.

It was voted that the following motion offered by Professor Albert Sauveur be also referred to the American Branch of Committee No. 1:

"That the American Branch of Committee No. 1 be instructed to ascertain whether, under the manufacturing conditions prevailing in American rail mills, the first rail rolled from

each ingot is generally defective owing to the presence of unsound metal resulting from the pipe or cavity which, under normal cooling conditions, forms in the upper part of every steel rail ingot, and, if such defects are found to exist, to introduce into the specifications such stipulations as in their judgment will best prevent the rolling of these defective rails."

The Chairman then opened the discussion on "The Relation between the Basic Open-Hearth Process and the Physical Properties of Steel," which was participated in by a number of speakers.

Mr. Paul Kreuzpointner read a paper on "The Ethics of Testing," and Professor Gaetano Lanza a paper on "Steel Rivets."

Mr. William R. Webster then called attention to the following criticisms that had been made on the Standard Specifications, in their discussion before other societies, some of which the speaker thought deserved careful consideration:

(*a*) SPECIFICATIONS FOR STEEL FORGINGS:

1. Forging specifications should be subdivided to be made applicable and useful as part of a contract. They now cover too many grades of steel and tend to confuse.

2. Upper limit of tensile strength should be specified for each grade of steel.

3. In some classes of forgings, 8-inch test pieces could be used to advantage in place of the 2-inch test pieces necessary in many classes of forgings.

(*b*) SPECIFICATIONS FOR STEEL CASTINGS:

1. Particular stress has been laid on the great importance of annealing, and reference has been made to many castings that are put in service unannealed.

2. Elastic limit is too low, should be one-half ultimate strength. None of the tabulated specifications give the elastic limit as less than one-half the ultimate strength.

3. 2-inch test piece is not necessary for steel castings, 8-inch test piece is far preferable.

(*c*) ELASTIC LIMIT:

It is not considered necessary to specify both yield-point and elastic limit, as the latter is broad enough to cover all needs. It has been claimed that to determine the elastic limit accurately, measurements have to be taken to the 10,000th of an inch with delicate instruments.

On the other hand it has been claimed that the yield-point can be taken by the drop of the beam. But check tests by the ordinary methods in use showed that the beam did not drop until the test specimen had elongated over 0.2 in. in 8 inches. A reasonable mean between these two extremes would seem to be that prescribed by Mr. Theodore Cooper in his Specifications for 1901, viz:

"For the purpose of these specifications the elastic limit will be considered the least strain producing a visible permanent elongation in a length of 8 inches as shown by scribe marks of a pair of finely pointed calipers."

"If the yield-point or drop of the beam can be calibrated for any machine and its speed to represent the elastic limit within 5% it may be used for general cases. Test reports must state by which method the elastic limit was determined."

On motion, the consideration of these criticisms was referred to the American Branch of Committee No. 1.

Mr. J. W. Whitehead, Jr., offered the following resolution, which was adopted:

*Resolved*, That the Chairman of this Convention appoint a Committee of at least six members of this Society for the purpose of gathering statistics, making experiments and testing in numerous ways the different materials offered for sale in the open market for the purpose of protecting steel against corrosion, and for recommending some means of disposing of the mill scale without causing unusual expense and delay to the assemblers. Also to recommend a set of Specifications for the application of the preservative to the steel, same to be reported at the next Annual Meeting.

The meeting then adjourned till 8 P. M.

---

## FOURTH SESSION, FRIDAY, JUNE 13, 8 P. M.

### ON CEMENT.

This session was in the form of an "Engineering Smoker," the President, Dr. Charles B. Dudley, occupying the Chair.

The following papers were read and discussed in the order named:

"The Correlation of Cement Specifications," by Mr. Robert W. Lesley.

"The Advantages of Uniformity in Methods of Testing and Specifications for Cement," by Mr. George S. Webster.

"The Chemical Analysis of Cement: Its Possibilities and Limitations," by Mr. Richard K. Meade.

"Cement Testing in Municipal Laboratories" by Mr. Richard L. Humphrey.

"Tests of Reinforced Concrete Beams," by Professor William K. Hatt.

It was voted that the following motion offered by Professor William K. Hatt be adopted:

That the Executive Committee be requested to consider the advisability of appointing a committee to report on "Specifications for Paving Brick and Building Brick."

Favorable action was also taken on the following motion by Mr. Robert W. Lesley:

That the question of appointing a committee to prepare "Standard Specifications for Cements" be referred to the Executive Committee for such action as it may deem proper.

Thereupon the meeting adjourned till the following morning.

---

## FIFTH SESSION, SATURDAY, JUNE 14, 10 A. M.

### ON CAST IRON.

President Dudley in the Chair.

A paper by Mr. W. G. Scott on the "Effect of Variations in the Constituents of Cast Iron," was read in abstract by the Secretary, in the absence of the author, and afterwards discussed.

Then followed the reading of the following papers in the order indicated:

"Present Status of Testing Cast Iron," by Dr. R. G. Moldenke.

"The Need of Foundry Experience for the Proper Inspection and Testing of Cast Iron," by Mr. Thomas D. West.

A Quick and Automatic Taper-Scale Test," by Mr. Asa W. Whitney.

"High Strength of White Iron Castings, as Influenced by Heat Treatment," by Mr. Alex. A. Outerbridge, Jr.

Mr. Walter Wood offered some informal remarks on "Specifications for Cast-Iron Pipe," and a general discussion of the above papers then ensued.

Mr. James Christie stated that in pursuance of a suggestion from the Secretary-Treasurer, he would move that an Auditing Committee be appointed to examine that officer's accounts. This motion was carried.

There being no further business, the President declared the meeting adjourned *sine die*.

# AMERICAN SOCIETY FOR TESTING MATERIALS.

AFFILIATED WITH THE
INTERNATIONAL SOCIETY FOR TESTING MATERIALS.

# PROCEEDINGS.

The Society is not responsible, as a body, for the statements and opinions advanced in its publications.

## ANNUAL ADDRESS BY THE RETIRING PRESIDENT.

HENRY M. HOWE.

It seems well that your retiring President should enunciate briefly his understanding of the chief aims of our organization, and summarize the chief events which have taken place during his term.

What is our chief aim and object? We are members of the International Association for Testing Materials. The testing of materials has two chief features, first, the manner in which tests shall be conducted, and second, the specifications which the materials tested must endure. Of these two features the European members of the International Association in general regard the former—the manner of conducting tests—as the primary object of the Association, and the second—the reception specifications—as a thing for later consideration. But we American members, I take it, look at the matter from a radically different standpoint, and look to the erection of normal or standard reception specifications as by far the more important object. It appears to us that buyer and seller have greater difficulty in agreeing on the specifications which the material bought and sold must fulfill, than in agreeing on the methods to be used for deciding whether those specifications, once agreed on, are fulfilled by a given lot of material. We do not expect, nor do we think it important, that the standard specifications which we erect shall be exactly complied with in many, or indeed

in any, actual transactions. But we are firmly convinced that the existence of such standard specifications, stamped as standards of reasonableness by a properly constituted body such as we aim to be, will greatly facilitate agreement between buyer and seller, will help them to come to terms, to convince each other that this or that item specified by one of them and objected to by the other is or is not reasonable. Reasonableness should be the spirit in which every negotiation is carried on; standards of reasonableness such as we wish to erect should greatly aid reasonable men in deciding what is reasonable. While we believe this true of domestic negotiations, we believe that it applies with even greater force to international negotiations, between buyer and seller who have never seen each other, who live under different conditions of society, with different traditions, conceptions, and points of view.

No words are needed to show the great importance of this field of work. It is well that there should be some society with this as its chief object. Such a society should have very strong international affiliations; these we have. It should represent both sides of the negotiations, seller and buyer, maker and user. It should represent also what we may term their professional counsel, the consulting and inspecting engineers. If I may still farther borrow the terms of litigation, it should represent also the bench, that is to say the institutions of learning, through their teaching and investigating staff, and the engineering press. These three classes, I repeat, first, the negotiators themselves, maker and user; second, their counsel, the testing and consulting engineers; third, the disinterested and quasi-judicial class, the teachers, investigators and editors, should be fully represented in our membership and our Executive Committee.

This most important field has been that of our Committee No. 1, whose admirable work has commanded such applause. This same work should be applied to the other chief materials of construction, such as cast iron, cement, brick, brasses and other alloys.

This work never ends, though its continuation calls for less outlay of energy than its initiation. Specifications will ever become more rigorous, more exact; our Committee No. 1 should continue indefinitely modifying its standards to bring them into

harmony with existing conditions and possibilities, and to make them not simply reflect those conditions but aid materially in developing them. In short, we should aid and lead in the development of specifications, leading and shaping custom itself so far as is possible.

Next in importance is the work of improving the methods of testing. All these methods are indirect; their evidence is only circumstantial. We insist that rail steel shall have a certain high degree of ductility as shown by its elongation and contraction of area, although the rail itself will never be called upon to display in actual service any ductility comparable with that on which we justly insist. This elongation which we require, then, is not to give ductility, in itself a useless property for a rail, but to serve as indirect and circumstantial evidence that the material has the needed shock-resisting power, because experience has shown that this power will exist if the ductility exist. Theoretically every reception test should approach as closely as possible the most trying conditions of actual service. That they at present do not, is due in large part to the extreme crudity of our knowledge and practice, and in large part to the great difficulty in reproducing in our tests the conditions of service. They should be made to approximate more and more closely to the actual conditions of use. Here, too, we should at once reflect what is best and most advanced in existing practice, and so far as possible direct practice into better and more reasonable methods of testing.

A third important object, though one of less vital importance, is the publication of investigations of new products, and of the effects of mechanical and thermal treatment of iron and steel, and of like subjects; in brief, of those scientific questions which affect the great materials of construction to such a degree that provisions touching them ought to form part of reception specifications.

Turning now to the events of the past two years and the steps now proposed, four of these justify special mention here. They are,

The cordiality of our relations with the administration of the International Association.

Our good fortune in securing a permanent working Secretary.

An appeal for funds to the great makers and users of the materials of construction.

The change from the condition of a Section of the International Association to that of an independent self-directing, but closely related, Society.

If, through causes which are best forgiven and forgotten, the understanding between the Administration of the International Association and its American Section was not so clear as could have been desired, the obvious step towards clarifying it was to bring about personal interviews. The Buda-Pesth Congress afforded a convenient opportunity, and three of our members, including your now retiring Chairman, availed themselves of it. As a result of this I believe that the most cordial feelings have been established on both sides. At this I rejoice, because this cordiality is an absolute necessity to efficient coöperation in our joint work.

In every scientific and technical society there should be at least one permanent officer, in order that not only the policy of the society may be continuous, but also that its work may be efficient. Experience shows that this officer should be the Secretary, and that he should be the executive officer who, with the advice of the President and Executive Committee or Council, should not only execute but also shape the policy. Presidents come and Presidents go; generally they are King Log; rarely, fortunately, they are King Stork. Their natural function seems to be to accept office both as a high compliment to themselves and as giving a chance to aid by their advice the growth of the society, and perhaps, if they be of force, to leave some impress of their character. But the active working man should be the Secretary. The Society will be what the Secretary makes it. In order that the policy of a society such as we aim to be should develop with proper promptness, and that its affairs should be administered with efficiency, the Secretary should be able to give a very large amount of time to its work. This practically means that he should be salaried. The first duty that pressed upon my administration was to find such a one, a man who could spare the needed time, who had the needed tact, administrative talent and breadth of view, and also—and here is the rub—who occupied a judicial position, between maker and

user. If I am right in saying that the society is what the Secretary makes it, and if the society is to perform a judicial function, setting up specifications which shall be standards of reasonableness as between maker and user, then it is imperative that the Secretary himself shall not be identified either with the manufacturing or the consuming interest. This eliminates at once probably three-fourths of the available men. It leaves us the teaching and investigating staffs of our institutions of learning, the editorial staffs of our technical press, and perhaps a few independent investigators.

Your Executive Committee labored for months with this question, and as a result I am confident that in our present Secretary, Edgar Marburg, Esq., Professor of Civil Engineering in the University of Pennsylvania, we have found the man who fulfills all the requirements which I have set forth. For him I predict confidently a successful administration of the affairs of this society.

Whence the funds for a Secretary's salary and expenses? As this society in effect serves these two great classes directly, the makers and the users of the material of construction, why should we not appeal to them for the funds to enable us to serve them efficiently? Of this appeal we have made a hopeful beginning.

As is explained at greater length in my report of the Buda-Pesth Congress, the President and Council of the International Association seemed unalterably opposed to the formation of national sections, and insisted that national or other local organizations of members of the International Association should be, not sections, but independent organizations, of which preferably every member should be also a member of the International Association. When we asked for the recognition of the American Section, as a measure of independence, they in effect replied, make your independence complete. As the greater includes the less, we may hold that we have the essence of what we sought. To be sure, we also sought that our independence should be coupled with the name of American Section of the International Association for Testing Materials. But few of us care for the name provided that we get the substance, for the shells provided that we get the oyster, for appearing to have our way provided that we actually have it in effect.

That our independence should be complete seems very desirable on account of the extreme divergence between the policy of the majority of the European members of the International Association for Testing Materials and our own policy. They regard methods of testing as the subject of paramount importance, and the erection of standard reception specifications as wholly secondary; indeed, a very large fraction of our European colleagues seem strongly opposed to even raising the question of standard specifications. We regard the question of standard specifications as of paramount importance, as indeed, an important world-question, and we regard the question of methods of testing as of a much lower degree of importance. With such a divergence of views, serious conflicts are to be apprehended if we owe to the parent society the allegiance of even a limited suzerainty.

Your Executive Committee therefore has adopted and will recommend to you the plan of changing our condition from that of a section of the International Association to that of a quasi-independent though very closely affiliated society, of which every member must necessarily be a member of the International Association for Testing Materials; though members of the Association resident in America need not, if we be abhorrent to them, join our society. Few such will probably be found.

This then is our record of things done or begun, the record on which we must have your approval or disapproval.

Harmony with the parent society established.

The establishment of a permanent working secretaryship.

An appeal to the industries which we serve for the sinews of war to make our service effective.

The change of state to that of an independent affiliated society.

What we have done and begun we believe are essential to the efficiency of the society; our service then we believe has been to devise and establish an efficient working scheme. The actual working of that scheme we must leave to our successors, to whom God speed.

# PROPOSED MODIFICATIONS OF THE STANDARD SPECIFICATIONS FOR STEEL RAILS.*

## TOPICAL DISCUSSION.

### (a) Drop Test.

The President. THE PRESIDENT.—The question before you is this: the standard specifications for steel rails, adopted by this Society provide that one drop test shall be made on a piece of rail chosen from every fifth blow. The American Engineering and Maintenance of Way Association proposes to change that feature by prescribing one drop test for every blow. The question for discussion is: Shall a drop test be specified for every blow or for every five blows? I think that is a question on which a good deal can be said.

Mr. McLeod. JOHN MCLEOD.—I believe it would not be desirable to make any changes in the specifications adopted by our Society. These specifications represent what they were proposed to represent, that is, standard American practice of to-day. This proposed requirement of the American Engineering and Maintenance of Way Association may become standard practice; when it does it will be in order for us to change our specifications. I believe further that the standard specification of the future will be different from our standard specification of to-day, for the reason that a radical change has been made in the process of manufacture of rails which is now on trial; and we think, if the new process is a success, that the carbon limit, for instance, can be changed and will be changed voluntarily by the engineers. Therefore, to depart at this time from what we have undertaken, that is, to produce a specification representing present American practice, would be unwise.

This question of having a drop test from every blow or from

* These standard specifications were adopted by letter-ballot of the Society on August 10, 1901. The American Engineering and Maintenance of Way Association, at its annual meeting in Chicago, March 18, 19, 20, 1902, adopted specifications for steel rails, departing from the aforesaid Standard Specifications in certain particulars (see pp. 9–10 of this volume.) These modifications were brought to the notice of the American Branch of International Committee No. 1, and it was decided to submit the same to the Society for general discussion.

Mr. McLeod. every fifth blow was taken up by our committee and thoroughly discussed. It was a compromise on the part of both the manufacturer and the engineer; when the committee decided to take the middle ground, by specifying one drop test from every fifth blow.

Since the pig iron goes into the mixer and thence into the converter to be converted into steel, the process is a continuous one, and one test from every fifth blow should furnish a complete check. If the material from one blow should happen to be unsatisfactory, there is a provision in our specifications which allows you to go back successively to the preceding blows until you find where that bad steel started; and you can likewise go to the succeeding blows consecutively until you find where it stops in that direction.

I believe these specifications are fair; and I believe that it is not dignified to be continually changing. We have certainly issued our specifications, and by so doing and discussing them have shown to the public that they represent our opinion. Now if they represent our opinion, why change them in less than six months after we have expressed that opinion? I believe it would be unwise to make a change and, as a member of Committee No. 1, I should dislike very much to see that change made.

Mr. Campbell. H. H. CAMPBELL.—My objection to the test for every heat is this: as Mr. McLeod has said, most people know steel rails are pretty nearly uniform, so far as chemical composition goes, in our steel works to day. I think that those who are in the inspection business know that from heat to heat the changes in carbons, etc., are not worthy of very much serious consideration. What we are after to-day is the heat treatment, apparently; all the discussion seems to center upon that portion of the work, on the microscopic structure of the steel, on the physical characteristics. I make the point that testing one bar from every heat does not settle that question in any way. If each separate heat were treated on a different day, or a different month, or a different hour under different conditions, then it would be worth while to investigate those separate lots treated under different conditions; but when a dozen heats of steel are in the furnaces and under the same treatment at the same time, when they are

going through so fast that it is almost impossible to distinguish one heat from another, when they are all on the beds together and are practically one and the same thing, why pick out one bar from each heat to test? If you are after the physical structure and characteristics you do not find out anything more by testing one from each heat: we know nothing at all about the remaining bars from that heat. For instance, if the blooms are being reheated, then one heating furnace may contain parts of two heats and the remaining parts of those two heats may be in another furnace. We have had a great fight over a clause in specifications requiring a test from each heating furnace; and yet there is just as much sense, I was going to say more, in having a test from a heating furnace for a bloom than from a heat made in the Bessemer; but we don't want both, and I think it is unnecessary in rail practice to have even the one from each heat; because if you get back to the heating furnace, then you should have one from each end of a bloom, because it is so very easy in the heating furnace to overheat one end and not the other, and everybody knows that if you overheat it at all the chances are it is only on one end and your rail crop may be cut from the other. When you test one end of a rail you practically know nothing about the other; because one end will be all right, even if the other end is overheated. Moreover the blooms in one end of the furnace may be overheated and not in the other, so that if you really want to know about the physical characteristics you have got to test both ends of every bloom and of every rail, which of course is entirely out of the question, so that the only thing to do is to test a sufficient quantity of steel to know how the steel is running in the average product of that mill, and I think that is determined by testing one out of every five heats. Mr. Campbell.

ROBERT JOB.—I suppose that we shall all agree that safety is the essential feature in a rail. We have found as a general thing that the condition which results in fractures under a drop test is not so often the granular structure of the rail, as the presence of foreign matter and gases, or, in other words, anything which breaks up the continuity of the steel. The main thing to guard against, then, in order to have a safe rail is foreign matter and enclosed gas. In following out the ordinary mill practice so great a variation in quality can come in between the different Mr. Job.

Mr. Job. heats, and throughout the heats, that testing only one heat in five is giving a great deal of latitude in margin of safety, and our experience has shown that it is exceedingly undesirable to have any such arrangement. We find one heat, for instance, which gives excellent results under drop test; the next heat may show complete failure, merely on account of the foreign matter which is present; perhaps the next one will be a failure, or possibly it may be a good one. Sometimes we find the result runs in one direction and sometimes in another, and there is no definite certainty regarding it. Upon that account, we consider it unsafe at times to allow the latitude given by a system of testing only one heat in five. Therefore it seems to me very desirable that our specifications should conform to what I think we can say is the best standard practice to-day, specifying a drop test upon a butt from every heat. It is the practice in many mills to-day, and by a good many railroad companies, and is the practice abroad. I think, also, it is no more than fair and just to consumer and manufacturer alike, to have this test from each heat, as a matter of protection to the mill, to the consumer, and to every patron of the road.

It is said to be the practice of the London and Northwestern Railway to take one rail from the top of every heat and to subject it to a drop test of one ton falling thirty feet. This, I understand, is the general practice.

Mr. Campbell. MR. CAMPBELL.—Does the speaker state that that is the general practice abroad?

Mr. Job. MR. JOB.—I might withdraw the statement, "general practice;" I shall merely cite it as an individual case stated recently by Sir Lowthian Bell to be the practice of the London and Northwestern Railway.

Mr. Campbell. MR. CAMPBELL.—One of the most stringent specifications is that of Sir Alexander Meadows Rendel, and this calls for a test on one rail for every two or three hundred tons. This is picked out of the pile at random, and the whole lot is accepted on the strength of this one test. It is two or three years since I have had any experience with these specifications, but they apply to the rails purchased for all the state railways of India, and the rails abroad are very much softer than they are here, and will stand a much greater shock.

WM. R. WEBSTER.—Sir Alexander Rendel specifies a 30-foot rail placed on supports three feet apart. The fracture often takes place outside of the points of support. The test is not as severe as on a shorter piece as the arms hold it up. Sir Rendel had his assistant at the India State College look up the method that we are using in this country, with the idea of adopting it, because his method was not satisfactory. They found English rails in India had been very unsatisfactory; but I am glad to say that the American rails sent to India, from the Maryland Steel Company, have given very good satisfaction. Mr. Webster.

MR. CAMPBELL.—I do not want to be understood as belittling Sir Alexander Rendel's specifications. I only want to say it is not the universal practice at all. Mr. Campbell.

HENRY M. HOWE.—Do I understand that the failures in rails have been traced to the presence of certain foreign matter in the steel, to wit: slag, oxide, and certain gases? If so, that is a very remarkable thing; and I would like to know whether those gases have actually been determined—whether the presence of that slag and those oxides in .50 carbon rail steel has been shown—whether, by actual analysis, those oxides and slags and other foreign matter have been shown to exist and to be the cause of rupture. Mr. Howe.

MR. JOB.—The way in which we get at that is by taking a rail section which has been fractured under a drop test, polishing off the entire section, and etching it lightly. I think it immaterial what etching medium is employed; we have used a strong solution of iodine in alcohol; and on etching have found the presence of a considerable amount of foreign matter and gas in cases where there has been a fracture of the rail. Mr. Job.

MR. HOWE.—I should rather think that it would be unsoundness or sponginess rather than presence of foreign matter. Mr. Howe.

MR. JOB.—The point is, it is not a solid homogeneous material, and the fact that it is not so necessarily implies the presence of foreign matter of some kind. Mr. Job.

ALBERT SAUVEUR.—I should like to say a few words concerning the location of the piece of rail selected for the drop test, as I think that it has a direct bearing upon the subject of this discussion. It has been a constant source of surprise to me to see what very scant attention has been paid by our Com- Mr. Sauveur

Mr. Sauveur.

mittee No. 1 in its specifications for steel rails to the very important question of what in mill parlance we call "piped" rails. Rail manufacturers, as you all know, discard seldom if ever over 10% of the total weight of their ingot. I asked them if they think that 10% is enough, to guard us against the production of "piped" rails. It is my experience, and I think it is that of many others, that the pipe extends much deeper into the ingot. I believe that in the present practice of rail makers the first rail rolled from each ingot is, to a greater or less extent, a "piped" rail, and therefore a defective rail, and I base my belief upon my examination during the last ten years of hundreds of rails that had proved defective. In the majority of cases I was able to trace the cause of the failure to the presence of "pipe," *i. e.*, of unsound metal in the head of the rail.

We should also remember that the present tendency is to allow more and more carbon in the steel, and that with the increase of carbon the tendency to form a "pipe" increases as well as the unweldability of the metal. It appears to me, then, if my premises are well taken, that the situation is a very serious one—one that should receive our careful consideration and the careful investigation of our Committee No. 1. It is, moreover, I think, a situation which most of us realize, but for some reason or other we have never faced it squarely.

If my fears are justified a clause should be introduced into the specifications to guard consumers against the rolling of those defective rails; and I think the best and most effective way to reach our end would be to prescribe that the piece of rail selected for the drop test shall be taken from the top of the ingot; that is, shall correspond to the portion of the metal immediately succeeding whatever the manufacturer cuts off. It is my belief that under our present practice a very large proportion of the pieces would fail under the drop test. Such specifications would necessarily compel the manufacturer to discard a sufficient amount of metal.

If the manufacturers claim that they only roll sound metal; if they claim that after discarding—whatever they do discard—the remaining metal is sound, then surely they should not object to have us select the top end of the ingot for testing. If they do object to it, it is because they feel or know that that rail is

weakest; and is it not a perfectly logical and sound policy to test the weakest, or what is likely to be the weakest, member of a series instead of the strongest member? It gives us an increased guaranty that the rails which have not been tested will come up to the standard. If we do not do so we encourage the production of many defective rails. Mr. Sauveur.

P. E. CARHART.—All our experience goes to show that one heat tested out of every five is sufficient to show the character of the steel. With reference to the point raised by Mr. Sauveur, I may say that it has been the practice of the Illinois Steel Company for the past ten years to select the drop test piece from the rail representing the first steel out of the top of the ingot. I believe that is also the practice of the other leading rail mills. Mr Carhart.

GEORGE E. THACKRAY.—Regarding the discard spoken of by Mr. Sauveur, he possibly has overlooked the point that this question is already well taken care of in the standard specifications for steel rails in which paragraph 1, clause (e) reads as follows: Mr. Thackray.

"Sufficient material shall be discarded from the top of the ingots to insure sound rails."

It appears that this is broad enough to fully cover the question of the necessary amount of discard, and as far as now understood, all of the rail mills in the United States conform to this practice.

To return again to the subject of making a drop test from every fifth heat of rails, some of the speakers seem to have forgotten that the standard specifications prescribe that in case this first test fails,

"two other rails will be subjected to the same tests, one from the blow next preceding, and one from the blow next succeeding the rejected blow." and "in case the first test taken from the preceding or succeeding blow shall fail, two additional tests shall be taken from the same blow of steel, the acceptance or rejection of which shall also be determined as specified above, and if the rails of the preceding or succeeding blow shall be rejected, similar tests may be taken from the previous or following blows, as the case may be, until the entire group of five blows is tested, if necessary." "The acceptance or rejection of all the rails from any blow will depend upon the result of the tests thereof."

From this it may be seen that in case of failures, every heat of the group of five will have been tested.

Mr. Thackray. Regarding certain statements made by prior speakers, you all know that the process of rail manufacture is a continuous and rapid one, and that for any given period the same class of metal is probably being charged into the Bessemer converter, while in modern mills a blow is made every six or seven minutes, so that under these conditions, there is no reasonable possibility of much change in the quality from heat to heat.

Under these conditions which now obtain in the modern process of rail manufacture, it seems that it is asking more than can be comfortably done to require a drop test of each and every blow, as this would only result in delay and disturbance of the rapid work. Although the modern process of rail manufacture is referred to as a rapid one, this does not mean that it is neglected, and the very fact that it is rapid, insures that it proceeds with regularity and uniformity, thus maintaining the same good qualities throughout any lot. This point was ably demonstrated by our retiring chairman, Professor H. M. Howe, in a paper read before the American Institute of Mining Engineers 1887 on the uniformity of Bessemer steel.

Mr Kenney. E. F. KENNEY.—As to the practice of the Illinois Steel Company with regard to the location of the test piece I have no personal knowledge, but at almost all of the eastern mills no attention is paid to that. As a matter of fact at reheating mills it is almost impossible to know what is the top of the ingot; it would require a private detective to watch the ingot in its passage through the reheating furnace. We find that one mill is so arranged that they cannot give us the butt; they have, in fact, refused repeatedly to do this. The present practice of taking one test from every five heats does provide for cases where the test piece fails under the drop, and the manufacturer is protected from condemnation of all the material in the other four heats by subsequent tests specified to be made, but the success of the one test piece means that the purchaser must accept, besides that heat, four other heats which have not been represented at all, and they may be very inferior metal, perhaps through overblowing, although the process of manufacture is a fairly uniform one; yet each distinctive heat may have had a different heat history in the converter, and for that reason one test may not fairly represent the whole five heats.

MR. JOB.—In one particular lot of rails which we had, taking five consecutive heats, the first one failed under the drop test; the next two heats were good; the next one failed, and the next one was good. Now, we have had instance after instance of taking a number of heats together which have given entirely different results within a range of five heats. Is it right, as a matter of protection to the traveling public, to make the guess—to pick one of those five heats at random, and say because the one is good, that therefore the other four are perfectly safe? In our tests it has not been shown so; in fact, the very reverse has been shown, and it has been proved beyond question that practically no reliance can be placed on the quality as a whole by testing only one heat in five. Mr. Job.

MR. CAMPBELL.—We ought to be very careful about the use of terms. I have protested privately to Mr. Job about the use of the term "foreign matter," which has just led to a misunderstanding with Professor Howe. I think it will lead to a misunderstanding in print with almost everybody. By "foreign matter" we mean a tangible thing like slag, or oxide of iron. Mr. Job is perfectly correct in stating that gas is foreign matter, but it is not so spoken of in our common usage. Moreover he states that failures were due to foreign matter, meaning by that that the rails which have failed have generally contained either foreign matter or blow-holes; but there is a lack of logical demonstration that the failure was due to those things. Now, a rail may be porous from many causes, and it may contain holes that are almost vacuous, or those holes may contain gas under pressure, or under no pressure. The conditions causing those three things are entirely different, and one of those conditions may make bad steel and another may make good steel. I have seen steel full of blow-holes which was perfectly good; I have seen steels full of blow-holes that were not perfectly good. I do not doubt but what some rails have failed because they were made under conditions which produced blow-holes, but when Mr. Job makes the statement that it is proven that these particular rails broke because they contained foreign matter and he means that some of that foreign matter was simply blow-holes, I wish to question the logic of the conclusion, because I think it is not proven. We simply know that it is an associated circumstance, Mr. Campbell.

Mr. Campbell

and I do not think that a rail specification should be based on a guess. It should not be assumed that there is a logical demonstration of causal connection when there may be only an associated circumstance.

Another thing ought to be considered very closely: I do not like to beg the whole question, but I object to the statement that the testing of a three-foot piece from every heat makes certain just exactly what we have, either for good or for bad;—it does not. If you want to find out, just test twenty pieces, forty pieces, and fifty pieces from one heat, and see if you will not sometimes get one broken one out of them. You have rails from the top of the ingot, rails from the bottom, and rails from the middle; you have tops from different ingots; you have got the last ingot that has some slag on it and possibly a little dirt, and it may be segregated a little way down; all the rails are not cooled under exactly the same conditions, not heat-treated in exactly the same way; I think we should be very careful and be sure that we have reached the final answer before we involve manufacturers in great expense in the development of new methods of testing. I believe that it is quite safe to presuppose a reasonable uniformity in the material from heat to heat.

Mr. Voorhees

S. S. VOORHEES.—With regard to the individual heat treatment of each rail, we had an experience some years ago which is rather conclusive. A lot of rails were bought prior to the installation of the inspection department, and went into service in Georgia. During the cold weather of February, 1898, one of those rails broke and caused a bad accident, and the rail was sent to us for examination. We found that the rail was hard and brittle. Chemical examination of borings from the top of the rail showed a normal composition: .4 carbon; manganese, about .8; phosphorus inside Bessemer limit—.098, I think. A section from top of head was machined and tested. It showed about 97 000 tensile strength, and no stretch at all, with a very coarse fracture. Another section taken off the other side of the head was annealed (heated up to cherry-red and cooled slowly in charcoal), and on testing showed a normal, 15% stretch. We did not have enough material for the drop test; but I have no doubt, from the way it behaved when it first came, that it would have failed under the drop test. A further inves-

tigation showed that the material was very seriously segregated, containing a large excess of sulphur and phosphorus; the phosphorus in the center of the web went up to .176. The failure, I should say, was due both to excessive segregation and to too rapid cooling from a higher temperature. That rail was rolled in November, if I remember rightly, and it was possibly one of the first ones rolled and went out on the cold bed. The fact remains that that rail failed. If that rail had been put under the drop test it would doubtless have failed; but the rest of the rails in that blow were perhaps all right. We have to take a good deal on faith; for we cannot test every rail to destruction. Mr Voorhees.

MR. JOB.—In connection with the matter Mr. Campbell just mentioned as to whether it was a matter of experience that rails broke on account of the presence of this foreign matter, an additional page of history may be of interest. At one time we had a large number of rails which broke under the drop test, and spliced in between those different heats were a number in which the rails did not fracture, and we investigated the matter very carefully to satisfy ourselves as to the definite cause of the fracture, or of the absence of the fracture, as the case might be. As far as the chemical composition was concerned, we found only very slight differences; that is, whatever differences there were, were so small that they could not have been the cause of any difference under the drop test. Sometimes the broken butt had the more favorable composition, and that which did not break had the worse of the two; although all of them were satisfactory in composition. Then we examined the microscopic structure and found that as far as the size of the grain was concerned, there was practically a similarity,—merely slight differences. When we carried the matter a step further we found that those rails which broke under the drop test, showed a surface which on etching was quite pitted with portions of foreign matter, under which term I include gas, as I consider that gas is a foreign matter in a rail. We also took rails which did not fail under the drop test; and in every case that we examined, and we examined a good many, we found that there was a band of clear, homogeneous material around the outside of that rail; in other words, the one which had broken, Mr. Job.

Mr. Job. which was brittle and went to pieces like glass under a single blow of the drop test with a given composition, had these defects. The extent of the latter was more or less pronounced; sometimes they extended into the centre; sometimes they were merely upon the outside; but we always, in all of those broken rails, found indication of this foreign matter and gas in the steel; and in the case of those which did not break, of the same composition, and under the same conditions of rolling, we found a band of homogeneous metal around the outside. It looked to us at that time—and it does so now—after following out the investigation more thoroughly, that the effect of each condition upon the strength was definite.

Mr. Thackray. MR. THACKRAY.—Apropos of the subject of foreign matter having such a preponderating influence upon the quality and wear of steel rails it seems that this is something new, and rather radical, and we ought to have considerably more light upon a subject of this kind before making any conclusions so broad and sweeping, based upon such slight premises. It might be well to remember that, apart from the tensile strength of the material the ability to stand a drop test, for example, is very well shown by the old-fashioned wrought iron which is full of foreign matter and possesses a great many of its good qualities by reason of the fact that, instead of being a homogeneous material it is a bunch of fibers separated by foreign matter, and each fiber when deformed by bending has the opportunity to slide, just as the strands of fiber of a rope would under similar conditions, and it does not break. This is merely given as a rough illustration of what foreign matter has done for wrought iron.

Mr Carhart. MR. CARHART.—Referring to the drop test, for eight or nine years it was the practice of the Illinois Steel Company to make one drop test on every heat, the requirements being identical with the ordinary specification that two out of three tests shall stand. The number of failures were, however, so exceedingly small that our company decided that it was unnecessary to continue this practice. Our practice to-day is in keeping with the present standard specifications. I certainly think, and I believe you will agree with me, that eight years' experience is pretty fair evidence.

Mr. Howe. MR. HOWE.—The expression "foreign matter" for this sponginess appears to me a misleading one. Certainly it completely

misled me; and if it misleads one it may easily mislead others. From the reference first made at this meeting to these things, I understood that it had been shown that dissolved or occluded gases, and slag in microscopic particles such as exist in wrought iron, for instance, and to a minute degree in steel, caused rupture. I now understand that it is nothing of the kind, but spongy or unsound spots, due initially to blow-holes, pipes or some like cause that were referred to. Such sponginess, I think, is not well described as foreign matter. Empty cavities should not, I think, be spoken of as gases. Such cavities, may be, have been formed by gases in which case they are not gases but sponginess left by gases; or they may be due to contraction, in which case they may have nothing whatsoever to do with gases. Mr. Howe.

The term "foreign matter" might naturally be thought to mean elements like manganese, silicon, phosphorus, graphite, etc., elements which are often incorrectly grouped together as "metalloids;" incorrectly, because manganese is about as metallic as a metal can be, and has nothing metalloidal about it. Those of us who speak accurately, long ago rejected this term metalloids for these elements, and speak of them as the "foreign elements." This expression is well established; and to coin a new expression "foreign matter" so closely like it in sound, to cover the wholly distinct class of things, sponginess and slag, seems to me most unfortunate both because very likely to be misunderstood, and because it seems wholly improper to speak of empty cavities as "matter."

Turning now to a different subject, I think that the demand for testing every heat rests in large part on a confusion of ideas, on the idea that each heat is a homogeneous whole, and that if we test each heat then we are nearly sure to detect all bad steel. But this is not at all the case. A given heat may yield much steel that is good and some steel that is bad. A given heat is by no means a homogeneous whole, alike in all its parts. The different ingots are cast under somewhat different conditions; they may be treated very differently in the heating furnace, the finishing temperature may be very different, etc. Therefore, testing each heat is by no means in itself a guarantee that all bad steel will be detected, nor is it necessarily a rational basis for testing.

Mr. Howe.

It seems to me that the proper basis for deciding whether we shall test a piece from every heat, or a piece from every five heats, or a piece from every other heat, really is a question of reasonable frequency of test. The object of testing as I take it neither is nor can be to detect by direct test all bad steel with certainty, because in order to do that we should have to test every piece, every rail or every tire. The object is wholly different. It is to hold up a warning and a penalty to the manufacturer such as to induce him to regulate his manufacture with such accuracy and uniformity that all steel shall be good. The number of tests should be governed by the consideration that they must be frequent enough, not to detect every bad piece of steel, but to ensure that, if the manufacturer is careless or irregular, then from time to time this irregularity will be detected through these tests, and a prohibitory penalty will be inflicted on the manufacturer. This, it seems to me, is the true point of view, not to aim to detect directly every bad piece of steel, but to hold up a severe penalty to the manufacturer for irregularity, and then to have tests of such frequency that the manufacturer will realize that, if irregularity exists, if he even occasionally makes bad steel, the chance of detection and punishment is dangerously great, and the danger of the consequences is prohibitorily great.

If my view is right then it becomes purely a question of reasonableness how frequent these tests shall be in order to bring the manufacturer to act on this principle. To make the matter a little clearer let us take the position of the manufacturer: suppose that his manufacture is so irregular that every now and then a bad rail is made. Suppose that tests are so frequent as to lead him to foresee there is a prohibitorily great chance that some one bad rail will be detected, and will bring upon him an intolerable penalty. That throws upon him an irresistible motive to remedy that irregularity, and that motive is just as powerful as if he knew that every individual bad rail was to be detected. Our object as I take it is to bring about complete freedom from bad rails, or bad product of whatever class it may be. We cannot bring that about through detecting every bad rail. That is an impossibility. We must bring it about by providing a severe penalty, and then making tests of

such frequency that there is a clearly prohibitory danger that, if irregularity in the manufacture exists to such a degree as to lead even occasionally to bad rails, at least one of these bad rails will be detected, and that the penalty in that case will be intolerable. This danger should be so great as to force the manufacturer in self-defence to remedy the irregularity. Mr. Howe.

MR. KENNEY.—Is not that a little revolutionary? Has not the heat always been recognized in every mill in the country as a unit? I should like to ask Professor Howe that. Mr. Kenney.

MR. HOWE.—The answer is that we recognize the heat as a convenient unit, but that does not mean at all that every heat should be tested. We must decide upon some reasonable frequency of test and the most convenient method of expressing this degree of frequency is to say that we will take a test from a certain specific number of heats. This may be one test from each heat, or it may be one test from every ten heats, or one test from whatever number of heats it appears reasonable that we should select in view of the reasons which I have already stated. Mr. Howe.

MR. SAUVEUR.—If it be true, as Mr. Campbell has well said, that the result of one test does not prove that the whole heat comes up to the standard, still less does it prove that five consecutive heats are right. It only points that way. It increases our confidence in the metal if we test it and find that it stands the requirements; and if we make more than one test—if we make more frequent tests—it simply increases our confidence a little more; the more satisfactory tests we make the greater is our confidence in the metal. It is only a question of how many tests we can reasonably require without undue interference with the process of manufacture. It seems to me that that is the whole question. Mr. Sauveur.

MR. JOB.—Mr. Carhart mentioned the experience of his mill and the small number of failures which occurred under the drop test. If we had obtained such results running through our drop tests, I may say that it would be almost immaterial whether we made drop tests or whether we did not; for under such conditions we would be perfectly willing to admit the arrangement of one test out of every five heats. But unfortunately in our regular practice we have found a difference at different mills. Mr. Job.

Mr. Job. The good practice at one mill is not present at another, and in order to know definitely about the quality of the steel at some mills, we may find it necessary to take more precautions than might be necessary at others. We have found this practical difference, and we must remember, as was said in the first place, that we must have safety in our rails, and do whatever is necessary to ensure that condition. Now if a given mill is perfectly satisfied that its product will show very few failures, why should objection be made to one test from every heat? It would not cause the rejection of material as long as it were good, and I think it is no more than right to the consumer that he should be given a fair assurance where safety is concerned.

Regarding the question of "foreign matter" discussed by Professor Howe, I do not wish to coin any new term, but merely to express the known condition in such a manner as to be most clearly understood, and if that will be effected by saying that we find the presence there of foreign matter *and* gases, why, that will certainly be the proper way to express it.

We find some rails, for instance, that show this condition. Originally the material contained blow-holes with practically no oxide and no slag in them. In the process of rolling the blow-holes were drawn out, and practically nothing is observable except an unwelded seam. Of course, it was merely the presence of a certain amount of gas which prevented the welding of the metal. What would be the best method of defining this condition?

Mr. Howe. MR. HOWE.—I should call it an unsoundness. A spongy spot or an unsound spot.

Mr. Job. MR. JOB.—You think that the best plan would be to group that whole condition under the word "unsound;" that is, to include all unsoundness, whether caused by oxides, slag or gas?

Mr. Howe. MR. HOWE.—That question I am not prepared to answer finally without further consideration. I should think that "sponginess" or "unsoundness" would certainly mislead no one; whereas I think it is very evident that to call this "foreign matter" would surely be both misleading and inappropriate.

The sort of structure to which I understand Mr. Job refers may be due to blow-holes, or it may be due to the contraction in cooling which gives rise to the structural cavity or pipe. It is hardly proper to speak of it as due to slag, because slag is not a

cause of such sponginess but simply a concomitant. The slag which is present in very minute quantities in molten steel segregates towards the last freezing part, and therefore is found in the spongy region about the pipe. Mr. Howe.

MR. JOB.—If the term "unsoundness" will define the condition better, I should certainly wish to have that term used. Unsoundness, then, will cover gases, oxides, and slag, or whatever extraneous material is present in the steel. Mr. Job.

MR. MCLEOD.—I think the manufacturers have been misunderstood in their attitude towards this particular question and therefore I should like to explain, at least, the attitude of the companies I represent. We feel, and I believe the other manufacturers feel, that a test from even one blow in five is more than necessary; yet we find that we can do this without incurring the danger of paying demurrage on cars, and packing our shipping beds so that we cannot get the rails out of the way of the mill. It would certainly clog our mill, and be a difficulty for us to test every blow that we make. The rolling of rails is a rapid operation, and we are surrounded by all kinds of conditions which hamper the smooth running of the work. I object, and I think the other manufacturers will object, to more frequent tests, not through fear of failure, but through fear of other conditions which would arise because of so much testing. I think the engineers should be satisfied with the number of tests prescribed in the specifications adopted by this body some time ago. Mr. McLeod.

MR. CAMPBELL.—That is a point which should be brought out strongly. At a modern mill there is a heat rolled every five or six minutes. Now, if it is required that a drop test of each heat shall be cooled and tested, and a repetition made in case of failure, then there must be some pretty lively work night and day, for the rails have got to go through and get shipped and there must be no delay because that blocks the whole mill and yard. If we have a test every five heats it gives us a chance. Mr. Campbell.

PAUL KREUZPOINTNER.—Concerning the meaning of soundness or unsoundness and foreign matter in steel which has been raised here, I believe we have a right to speak about unsound steel though it may not be spongy or have any blow-holes. Steel has frequently come under my observation which was sound and Mr. Kreuzpointner.

Mr. Kreuzpointner.

homogeneous to a high degree, and yet it contained foreign matter to such an extent as to make it very brittle. Now the question simply resolves itself into what that foreign matter is. I remember one instance a number of years ago where a tire broke in service causing a wreck. The tire was sent to our place for examination, and as soon as I had fractured it under the drop I saw it was dirty steel; and before that examination was completed a second tire broke, and a third one, causing a great uproar. Of course, the manufacturer came on at once; and the matter was examined. At first glance the manufacturer declared that steel dirty. What made it dirty? There were no blow-holes nor sponginess, yet our examination and the manufacturer's examination declared that steel unsound and full of oxides and dirty to such an extent that the microscope was not needed at all.

It is a matter of fact, proved every day in the testing room, that so-called foreign matter may be present, and produce defects in the steel to such an extent as to make it very brittle.

Allow me to say also that we cannot, in justice to the manufacturer or the consumer, compare any foreign matter of whatever kind or extent, in its action on steel, to wrought iron. Wrought iron is entirely different in its behavior and structure from steel. The very material, namely the cinder, which helps to make wrought iron what it is, a fibrous, ropy metal, if introduced into steel, in minor quantities even, tends to make that steel unsound and unfit for use. Steel is crystalline, to begin with, and depends for its strength on the cohesion between the faces of the crystals. Any foreign matter present in steel acts like a wedge, so to speak, to drive the faces of these crystals apart. In wrought iron we have different conditions. The cinder, if it is not present in too great quantities, will act as a binder, so to speak.

The President.

THE PRESIDENT.—Perhaps we have threshed out this idea of frequency of testing sufficiently. I may be allowed, before calling you to record yourselves on the matter, to say just a word from my own experience. The question of how frequently a test shall be applied is one that has given us a great deal of thought; and I am frank to say to you that in our specifications it is largely arbitrary. In oil we test one sample out of a car-

load. In axles we test one sample out of a heat; but some steel works make sixty axles in a heat, some make thirty, and some forty-five. In bronze we test one sample out of 20 000 pounds; in phosphor-copper we test one sample out of 10 000 pounds; in sheet-tin we test one sample out of every ten boxes. Where safety is involved we use more care than where it is simply a commercial question. We use our best judgment and make our sampling arbitrary to start with when a specification is made. Then, we follow the matter up very closely, and try to learn what experience shows as to the value of frequency of sampling. Does the service indicate to us, that bad material is slipping through between our samples so that we ought to sample more frequently? If so we change the specification and take more frequent samples. Now it seems to me that this principle applies pretty well to the rail question. It has been very well brought out, I think, that to test every heat would practically block the mills, unless a number of drops were put up so that tests could be made as fast as the heats are turned out. On the other hand, is the frequency of breakage of rails such that those in charge should feel that they ought to test more frequently in order to get greater certainty? To my mind, that is the key to the situation. The President

There is also one feature more that Professor Howe brought out a little, and that I had hoped to see brought out a little more fully, namely: the moral effect on the manufacturer of any testing. I am going to say to you very frankly, we who use material do not deceive ourselves by thinking that because a shipment passes test, every bit of that is good material, but we do feel this: that the manufacturer will give more attention to his product, will, in short, make better material, if we occasionally test it, than if we did not test it at all; and especially, as Professor Howe has said, if, in case the test sample is not good, we make him pay the penalty. One of the great values of testing is the rejection, and the rigid rejection, of material that does not pass, whether the sample is exactly like the balance of the heat or not. It is the moral effect on the manufacturer of suffering the loss, as we look at it, that is one of the strong elements in the value of testing.

Mr. McLeod.—I think that since these specifications have been passed on by the Society, the proper course for us to take Mr McLeod

Mr. McLeod. to-day would be to refer this matter to the American Branch of Committee No. 1, so that that Committee may make such further investigation as it may deem proper and then, as a result of such investigation, may refer the matter back again to the Society for action, just as was done in the case of the original specifications. I would like to offer this suggestion in the form of a motion.

The President THE PRESIDENT.—Would it not be well to have, in connection with that motion, an expression of the sense of this meeting as to whether one drop test should be specified for every blow or for every five blows?

Mr. McLeod. Mr. MCLEOD.—I amend the motion of referring the question back to Committee No. 1 by adding that it is the sense of this meeting that one drop test for every five blows is sufficient. [This amended motion was duly seconded.]

Mr. Christie. JAMES CHRISTIE.—In connection with any criticisms made on our specifications, it would be well to remember that in formulating these and presenting them to the International Association, the Committee were not guided by the thought of presenting any novel or ideal specifications. The guiding thought was to prepare specifications that would represent as fully as possible current and usual American practice. In fact, the primal intention, as represented in instructions from the parent association, was to formulate standard methods of testing, based upon existing specifications. Therefore these specifications should not be considered as what they should be with a view to improvements, but rather as representing the existing conditions.

The President THE PRESIDENT.—If there is no further discussion I shall put the question. The motion is to refer the matter of drop tests back to the American Branch of International Committee No. 1 with the statement, that it is the sense of this meeting that one test for every five blows is sufficient. [Motion carried.]

### (b) HEAT TREATMENT.

The President. THE PRESIDENT.—The next question to be discussed is that of heat treatment. The American Engineering and Maintenance of Way Association have introduced the following clause relative to heat treatment in their specifications for steel rails:

"*Heat Treatment.*—The number of passes and speed of train shall be so regulated that on leaving the rolls at the final pass the temperature of the rail will not exceed that which requires a shrinkage allowance at the hot saws of — inches for 85-pound and — inches for 100-pound rails, and no artificial means of cooling the rails shall be used between the finishing pass and the hot saws." The President

Our own standard specifications for steel rails contain no clause respecting heat treatment. The question for your consideration is whether or not it is desirable to introduce such a clause.

MR. MCLEOD.—I do not believe that at this time any one knows enough on the subject to specify what the shrinkage should be. We have adopted what we think is right, after having made some experiments. We believe that we are right, although it is not necessary now to discuss whether we are right or wrong; the rails will tell that for themselves, and it will be some little time before they commence to talk. The railroad companies will then tell us whether the rails we have furnished under our present system of rolling are better, worse, or as good as those furnished before we had that system. We made a difference of an inch in shrinkage from the old system to the new; on the heavier sections the difference is one-eighth inch more than that. The ratio that should exist between the new and old practice of other mills is probably different. We might have been finishing a good deal hotter than some other mills when we took this question up. Mr. McLeod

I do not think that we should introduce a clause on heat treatment in our specifications as yet; because we do not know enough about it. Coupled with the heat treatment I think the chemical composition will have to be considered. A new specification will perhaps some day be drafted which will represent current American practice, and in which the carbon will be lower, and the heat treatment more in line with colder finish. Time alone can tell, and the rails will talk for themselves. In the meantime I think it would be unwise and undignified to make such a change in our specifications.

MR. WEBSTER.—As a member of the Rail Committee of the American Engineering and Maintenance of Way Association, I would like to say that our views coincide to a certain extent with Mr. McLeod's: since we did not feel that we knew Mr. Webster

Mr. Webster. enough about the subject to say just what the specified shrinkage should be in the specifications, but we wanted to call attention to the matter. You will notice that we did not adopt the shrinkage prescribed in the new specifications of the Pennsylvania Railroad Company, but left the amount of shrinkage blank. I think Mr. McLeod and the other manufacturers will agree that some account should be taken of the proper finishing temperature of the material, instead of finishing at almost any temperature, as some of them have been doing; although it may be premature to put it in the specifications at this time.

Mr. Campbell. MR. CAMPBELL.—The steel makers are looking into this question very hard, and we shall know more about it after awhile, but there is one thing on which some light should be shed. I would refer it particularly to Mr. Webster as a member of the Committee, and as he thoroughly understands the question. The specification reads as follows:

"On leaving the rolls at the final pass the temperature shall not exceed a given point."

How is he going to determine the temperature of the rail as it leaves the rolls? The cooling action is very rapid; there is water on the rolls; there is water on the finishing pass; the web is very thin; the flange is very thin; the flange is cooler than the head; the web is cooler than the head; the flange is cooler than the web; what does he mean by the finishing temperature? It is the head you are after. If the head is at a certain temperature, we don't care so much about the web; the web is still colder. What is the temperature: do you want the web, the flange, or the head? How are you going to determine it when we find out what you mean? A certain time elapses in taking that temperature; it is only a few seconds—a fraction of a minute—but that is very important, and also the method by which the temperature is determined. Another question is in regard to the shrinkage at the saws; how long a time is going to be allowed from the time it leaves the rolls until you saw it? That is different in the different mills, and if you are to make a time allowance you must specify the coefficient of expansion and get that down very accurately for every different temperature. It is a very important matter. Out in our mill we saw almost the instant it leaves the rolls; in another mill it is so

arranged that there is quite an interval. It may be ten seconds or longer. These things are not quibbles; they must be put down accurately, because tens of thousands of dollars are often involved in the acceptance or rejection of the rails upon one contract. Mr Campbell.

MR. SAUVEUR.—The best way to regulate the finishing temperature, until we have looked into the matter more thoroughly, would probably be to regulate the temperature at the beginning of the rolling; and to prescribe, possibly, a certain speed of rolling, for, of course, the finishing temperature of the rail depends directly upon these two factors. I am quite prepared to say, however, that I agree with other members in believing that it is too soon to prescribe means for regulating the finishing temperatures; although we all recognize that it is a very important question, which eventually must be solved. Mr. Sauveur.

MR. WEBSTER.—I am glad that Mr. Campbell has raised the point as to using the shrinkage as a check on the finishing temperature. The Pennsylvania Railroad Company specifies that there shall be no delay from the finishing pass to the hot saw. The delay under the new conditions will be no longer than the delay under the old conditions. Under the old conditions the allowance for shrinkage was about 7 in. in 30 ft. for a 100-lb. rail. Those rails, as a rule, did not give satisfaction. At present such rails can be rolled with 5 5-8 or 5¾-in. shrinkage. The measurement of the temperature is based on the distance between the saws and the length of the finished rail. The conditions being otherwise the same under the old and the new methods, the shrinkage is 1¼ in. less, indicating a finishing temperature several hundred degrees lower. The point that presents itself to me is this: shall we saw and finish heavy rails at the last rolling, under similar temperature conditions as for lighter sections; or shall we ignore the question altogether and continue to finish them at a very high temperature? It has been suggested to start the material at a lower temperature at the first pass. We will have to determine whether the rolls will stand this, and many practical points will have to be solved; but the main thing is, will the manufacturers admit that there is any possibility of improvement in rolling, and, if so, what is the best way of reaching it? Mr. Webster.

Mr. Webster.

A few years ago the Pennsylvania Railroad made no drop tests on rails, and specified nothing concerning finishing temperature. About three years ago they specified finishing at a dull red heat; but they found this useless, and have since specified the shrinkage. The shrinkage is an easy self-applying test, and one well worthy of consideration. We need not attempt to complete the specifications at once; but we should think about this matter. If the manufacturers will record the amount of shrinkage on each weight of rail and make drop tests, then if they find that a small amount of shrinkage gives a better rail, as indicated both by the drop test and in service, they will in time, no doubt, adopt this method. If, on the other hand, the consumers find that a large amount of shrinkage gives rails having a coarse grain and rails that are brittle, they are certainly not going to continue to accept such rails.

In view of the remarks referring to differences of position of hot saws in relation to rolls, etc., causing a greater or less delay in the time of cutting rails after they leave the rolls, I would suggest the following tables for recording the results, in order to arrive at the conditions under which rails are finished at the different mills:

MILLS ROLLING FROM REHEATED BLOOMS.

| MILL. | Shrinkage for Rail 30′ long. | | | | Est'd Shrinkage Roll to Hot Saw. | | | | Est'd or Observed Fin'g Temperature. | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 70* | 80 | 90 | 100 | 70* | 80 | 90 | 100 | 70* | 80 | 90 | 100 |
| | | | | | | | | | | | | |
| | | | | | | | | | | | | |
| | | | | | | | | | | | | |
| | | | | | | | | | | | | |

* The figures for 70-lb. rails are for 70-lb. rails and under.

A similar table is recommended for mills rolling direct from ingot.

Mr Campbell.

MR. CAMPBELL.—Mr. Webster asks whether the manufacturers will acknowledge that there is room for improvement. I do not speak for all the manufacturers; I speak for only one of

them, when I say that we acknowledge it, but that is an entirely different thing from acknowledging that the shrinkage is the criterion to take. We know that there is room for improvement in certain directions, but we do not know that the shrinkage is the one thing on which to base our measure of that improvement, and that is a very important distinction, because everything in the proposed specification seems to be based on shrinkage. Shrinkage and quality may be related, but to say that they are related directly and proportionately would be a very rash and, I think, a false statement, because there can be instances adduced of mills which have been doing the very best work (of course I do not mean our own mill) where the rails did not shrink proportionately; rails have been rolled under proper conditions and have been in proper form and have given proper results, where the shrinkage on successive rails was far from uniform. Mr Campbell.

In regard to any specification of starting the ingot at a certain temperature, I think it would be a radical mistake. The first point against it is that some mills reheat their blooms, and I do not think it is the business of this society, or of any railroad, or of any other society, how a rail is rolled. I think it is better to roll a heavy rail down from an ingot in one heat, but I would not so specify. If a manufacturer can find a better way of doing it by making it in two heats, let him do it. The main point is the test of the finished material, and the punishment should fit the crime. It may be better that the ingot should be started at a high temperature, because you may weld it and more successfully close up the flaws and put the thing into shape, but it should be cooled down at some point in the operation so that it can be finished properly, but this is the business of the manufacturer, and there is no reason why you should make him break his mill down deforming a great big ingot in a cold state if he can finish it properly in some other way.

MR. HOWE.—It would be unwise to attempt to regulate the finishing temperature through regulating the initial temperature of the ingot or the bloom, because that would shut out the work of the man who arranges to let his rails cool between his leading pass and his finishing pass. Mr. Howe.

Now as to the possibility of regulating the finishing tem-

Mr. Howe.

perature by regulating the contraction after sawing. At first sight many may say that we do not know enough about the relation between the two to permit us to regulate the finishing temperature in this way. It is true that our knowledge of the connection between them is not highly exact; it is not clear to me, however, that our knowledge is not sufficient for this purpose.

We often think that because we don't know everything we don't know anything; but that is like saying that, because I don't know whether a man is 5 ft. 11¾ in. or 6 ft. tall, I don't know anything about his height. I do know very much about his height: I know that he is a tall man; for many purposes this is enough, and the further question whether he is 5 ft. 11¾ in., or 6 ft. tall, is unimportant. In this case I know all that is necessary, even though my knowledge is not exact.

Now, is not the same true of this question of finishing temperature? Let us say that the interval between the finishing pass and the sawing is not to exceed a certain number of seconds, and that the manufacturers and inspectors agree as to what is a reasonable number of seconds to stipulate. Let us then say that the contraction subsequent to sawing is not to be more than a certain specified quantity. You will at first say that we do not know what quantities to specify. I admit that we do not know with extreme accuracy, and that we shall later know with much greater accuracy than at present; nevertheless, it is not clear to me that we do not know with sufficient accuracy to permit us to establish a limit of contraction which shall not be oppressive to the manufacturer and at the same time shall be a safeguard of considerable value to the purchaser. The manufacturer certainly would not insist on being allowed a contraction as great as 12 inches, nor would the purchaser insist that the contraction should not exceed say 3 inches, to take most extreme figures. Now, between these two extremes is there not some reasonable limit to which manufacturers will agree, and which will be a really valuable safeguard for the purchasers? No doubt, with greater knowledge and experience we will be able to set this limit so as to protect the purchaser much more fully without causing a hardship to the manufacturer; but because we cannot reach perfection immediately is no reason why we

should not do what we can. I believe that there is some limit to contraction which we can agree upon to-day which is better than no limit at all. Mr Howe.

CHARLES L. HUSTON.—I am interested in this subject not merely as a manufacturer of rails, but in its bearing on the general question of rolling steel. Years ago it was quite frequently claimed that steel should have a certain percentage of reduction from the original ingot to the finished section, or thickness: if the finished thickness was a quarter-inch, the ingot should be so many inches thick to start with, and so on—everything should be in certain proportion. That has all been dropped now; and I think it is very much in line with the subject we are talking about. The rolling in its early stages, when the metal is hot, produces no permanent effect on the metal, for it is so hot that it does not retain the effect of the rolling. The final temperature is the important thing, of course. How that final temperature is reached is the interesting part of this question: whether one pass of 5% is sufficient after a certain time of cooling between passes to produce a permanent effect, or whether there is a temperature below which a permanent effect is produced upon the steel, also whether that temperature can be determined by experiment and a certain percentage of reduction shall be specified after the steel has fallen to that temperature. Mr. Huston.

MR. CAMPBELL.—It seems to me that this discussion might be more profitably continued this afternoon in connection with the discussion of the papers by Mr. Sauveur and Mr. Martin on rail temperatures. I therefore move to postpone the discussion till that time.* [Motion carried.] Mr. Campbell.

### (c) LENGTH OF RAILS.

THE PRESIDENT.—That leaves one question, namely: Shall the specified standard length of rail be thirty feet (30′) or thirty-three feet (33′)? The President.

MR. WEBSTER.—I move to refer that question to the American Branch of Committee No. 1. [Motion carried.] Mr. Webster.

*For a continuation of this discussion see pp 85–96

# IS IT DESIRABLE TO SPECIFY A SINGLE GRADE OF OPEN-HEARTH STRUCTURAL STEEL FOR BRIDGES OF ORDINARY SPANS?

Topical Discussion.

By Alfred P. Boller, T. L. Condron, Theodore Cooper, J. E. Greiner, John McLeod, C. C. Schneider and J. P. Snow.

Mr. Boller.

Alfred P. Boller.—The question as to the "desirability of adopting a single grade of open hearth steel for bridges" is a perennial one, and crops up naturally from the tendency to standardize manufactures and processes, which is characteristic of mechanical development. It is further fomented by the mills, which are more concerned with output than quality, and whose greatest efforts are to run continuously upon the grade of steel most productive of economic mill results. There is a natural and proper antagonism against fitful specifications, that some engineers deem desirable to make, if only for the purpose of laying down some different requirement from ordinary practice, as if to show originality of judgment. This, undoubtedly, causes a great deal of unnecessary trouble and expense to the mills, making them most strenuous for a consensus of engineering opinion, which will result in greater commercial ease and comfort. Now in this, the mills are not wholly wrong, but they are inclined to go too far in ignoring the engineering requirements, just as engineers may be unreasonable in establishing finicky distinctions without material differences. While the engineer should not abrogate to the mill the settlement of the grade of steel he should use in his bridges, still he should simplify his requirements within reasonable limits so as to combine the maximum of efficiency with minimum cost, which cannot be done through ignoring mill methods.

It is well, then, that such a body as the International Association for Testing Materials should step in and gather the best information and experience in the matter, which will naturally tend to uniformity of practice, and harmonize the requirements of engineers with mill possibilities.

The special topic under discussion seems to embody the

hopes of the mills, which can with advantage be nearly met for ordinary spans bridges, and can be answered in the affirmative, excepting as to rivets and eye bar material. There is no reason why plates and shapes should not be of a single grade open hearth for such bridges, but eye bar material, subject as it is to the subduing effects of forging and annealing, should be of a higher grade material. For large spans, which are rarely, if ever, committed to the care of other than expert bridge engineers, it is impracticable to set up any standard of uniformity, and the character and adaptation of the steel must be entirely in the hands of such engineers. These cases involve so many special considerations, that no rule can be laid down, and any engineer qualified to design and administer the construction of such works, is sufficiently well informed of mill possibilities as not to require impossible or impracticable results. Mr Boller.

It may be of interest, in this connection, to note the table of steel requirements adopted by my firm, and on which some thirty thousand tons of bridge material are now being gotten out for the Wabash Entrance into Pittsburg, for all types of construction; from the ordinary girder spans to the great cantilever constructions over the Monongahela and Ohio Rivers, where the limits of pin connected work are believed to have been reached.

This table shows a practical uniformity in all material, other than in the rivet, eye bar and pin requirements, and is believed to be as reasonable a demand on the mills as the present state of the art permits, and yet permits of the engineer exercising such judgment in selecting his material as the nature of the work demands.

| Shape. | Ultimate Strength per Sq. Inch. | | Elastic Ratio, Min. per Cent. | Elongation in 8 inches, Min. per Cent. | Reduction of Area, per Cent. | Chemical Composition Below. | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | P. | | S. | | |
| | Min. | Max. | | | | Acid. | Basic | Acid. | Basic | Mn. |
| Rivet | 52 000 | 63 000 | 55 | 28 | 50 | .04 | .03 | .05 | .05 | .60 |
| Angles and Plates | 60 000 | 70 000 | 50 | 24 | 40 | .06 | .04 | .05 | .05 | .60 |
| Eye Bars | 63 000 | 73 000 | 50 | 24 | 40 | .06 | .03 | .04 | .04 | .60 |
| Cast Steel | 65 000 | 75 000 | 50 | 20 | 30 | .08 | .04 | .05 | .05 | 80 |
| | | | | in 2 inches. | | | | | | |
| Pins | 65 000 | 75 000 | 50 | 22 | 33 | .06 | .04 | .05 | .05 | .60 |

Mr. Condron.

T. L. CONDRON.—At the third annual convention of the American Railway Engineering and Maintenance of Way Association held in Chicago this year on March 18, 19 and 20, the subject of specifications for rolled steel was presented in the form of a report of the Committee on Iron and Steel Structures. That committee, of which the writer has the honor of being a member, gathered statistics upon the subject of specifications in use, and sought the opinions of a large number of engineers and manufacturers. The replies received from forty correspondents showed the following opinions or preferences:

In favor of two grades of steel, "soft and medium":

Fourteen railroads, 7 manufacturers of steel, 9 consulting engineers, and 7 scientific schools.

In favor of one grade of steel: three railroad engineers:

One favored 55 000 to 65 000 lb. steel.

One favored 57 000 to 66 000 lb. steel.

One favored 58 000 to 68 000 lb. steel.

Notwithstanding the apparent general opinion in favor of two grades of steel, it was known to the committee that a large number of engineers limited themselves to one grade of steel in their bridge specifications. It was therefore proposed to the general committee, by the sub-committee having the matter directly in hand, that a recommendation be made by the entire committee, of one grade of steel for bridge building, to be known as "structural steel," having a range of ultimate tensile strength of 55 000 to 65 000 lbs. per square inch. This proposition was not favorably received by all of the committee, and the objections to such a recommendation were briefly these, as stated in the convention:

"The users of medium steel do not see why they should rule out what they believe to be a very excellent grade of steel between 65 000 and 70 000 pounds, if it is to be reamed, and place the upper limit at 65 000 pounds. It has been stated that the same class of service is required of practically all our bridges that are built, which is true; but the structures are built in two radically different ways by different railroad companies. A large percentage of the railroads entering Chicago, and our Western roads, are having their bridge material reamed; the reamed tonnage is constantly increasing and is a considerable proportion of the tonnage for our Western roads. On the other hand, some roads have not come to this reaming so generally. Of the two classes of structures one is the

class where the material is punched and not reamed, and the other the class where the material is punched and reamed, and the advocates of the two classes of steel are those who believe in reaming, and believe they can use a higher unit of strength where the material is reamed. They do not see why they should limit their working stresses to such a low point as is only permissible for work that is simply punched. This is the gist of the whole argument, and the reason why some of this committee have declined to agree on a 55 000 to 65 000 pound steel." Mr. Condron.

In the discussion of the subject, several members of the Association expressed the opinion that the one grade of 55 000 to 65 000 pound steel would be unsatisfactory for railroad bridges, as it would lower the strength of the steel unnecessarily, where reaming was done, and in order to obtain the general opinion of those present, a vote was taken which showed:

6 in favor of one grade, 55 000 to 65 000 pounds.

10 in favor of one grade, 60 000 to 70 000 pounds.

15 in favor of two grades, one below 60 000 and one above 60 000 pounds.

The majority of the engineers present refrained from voting at all, so that the above cannot be considered as a vote of the Association at large.

On June 10th last, this subject was again discussed at a meeting in Chicago by the following bridge engineers: Mr. Parkhurst (Ill. Central), Mr. Loweth (C., M. & St. P.), Mr. Finley (C. & N. W.), Mr. Dawley (C. & E. I.), Mr. Cartlidge (C., B. & Q.), Mr. Ziesing (Am. Bridge Co.), Mr. Allen (Wis. Bridge Co.), and Messrs. Modjeski, Schaub, Strobel, and the writer, consulting engineers. The general opinion expressed was that one grade of steel would be more desirable for structural purposes than two, both for the manufacturer and user, but that 55 000 lbs. is unnecessarily low for the lower limit. While all of these engineers are in favor of reaming of main sections, they realize that some roads are not doing so at this time, and will therefore wish to use softer steel than would satisfy those who ream. On the other hand, the steel used for buildings is nearly always the so-called medium steel, 60 000 to 70 000 pounds, which is not reamed. It is therefore likely that objection would be raised to reducing the strength of building steel below the present 60 000 pound minimum. If the engineer could be

Mr. Condron.

satisfied that the tests reported to him, fully, as well as fairly represented the physical properties of the steel he is using, he would have no objection to an extreme range of 10 000 pounds in ultimate tensile strength, but he realizes this is not the case, and with only the few tests (frequently but one for a heat of 30 to 40 tons) he must allow for a variation of four or five thousand pounds below or above the result of the tests reported. For this reason, he thinks he should restrict the range of tensile strength to eight thousand pounds instead of ten thousand pounds. Therefore, in the Chicago meeting above referred to, the general opinion was that a single grade of steel, having a range of tensile strength as shown by the ordinary tests, from 58 000 to 66 000, or 57 000 to 65 000 pounds as a second choice, would come the nearest to suiting all interests concerned so far as railroad work is concerned.

Regarding ranges of physical properties in any one melt of steel and the likelihood that steel varying 4 000, 6 000 or even 8 000 pounds above or below what any one test shows, attention is called to the results shown below in Tables A, B and C.

Table A shows the maximum and minimum results and the differences between these results from tests of plates of the same thickness, and from the same heat. These tests were made on a lot of 200 tons of basic open-hearth plates, rolled from 13 different heats into five different gages. It will be seen from Table B, which is condensed from Table A, that in the case of the ultimate strength the variations in one gage and one heat, were from 2 500 to 8 800 pounds, or, including the exceptionally wild heat "J," 12 300 lbs. per square inch. Similarly the yield point varied 1 700 to 12 600 lbs. per square inch, and the elongation varied from 3 to 12%.

In Table C, results of tests from another lot of plates are shown. In this lot, the variations of ultimate strength in the same gage and heat were from 1 300 lbs. to 8 500 lbs., and in yield point, 1 500 lbs. to 12 200 lbs., and in elongation from 1 to 6%.

These are not exceptional results, but are what is constantly observed, and if variations of 6 000 and 8 000 lbs. in ultimate strength are found in the same gage and heat, it must be conceded that as great or greater variations are to be found in

TABLE A. (VARIATIONS FOUND IN THE SAME HEAT AND GAGE.) Mr. Cochran

| Gage | Heat. | No. of Tests. | Tensile Strength. | Max. Dif. | Yield Point. | Max. Dif. | Elong. 8″—%. | Max. Dif. % |
|---|---|---|---|---|---|---|---|---|
| 3/8″ | E | 5 | Max. 54 900<br>Min. 51 400 | 3 500 | 31 600<br>29 900 | 1 700 | 32.0<br>26.0 | 6%* |
| | L | 6 | Max. 57 500<br>Min. 50 700 | 6 800* | 34 900<br>29 300 | 5 600* | 29.0<br>26.0 | 3 |
| | M | 5 | Max. 58 300<br>Min. 55 900 | 2 400 | 33 700<br>31 600 | 2 100 | 30.0<br>27.0 | 3 |
| | S | 1 | Max. 60 200<br>Min. 55 600 | 4 600 | 35 100<br>30 800 | 4 300 | 29.0<br>26.0 | 3 |
| 1/2″ | A | 7 | Max. 59 400<br>Min. 55 800 | 3 600 | 33 400<br>30 200 | 3 200* | 31.0<br>27.0 | 4 * |
| | K | 11 | Max. 58 200<br>Min. 51 700 | 6 500* | 34 100<br>31 400 | 2 700 | 30.0<br>27.0 | 3 |
| 7/16″ | C | 4 | Max. 56 200<br>Min. 53 400 | 2 800 | 33 000<br>31 300 | 1 900 | 33.0<br>31.0 | 2 |
| | R | 13 | Max. 59 000<br>Min. 52 200 | 6 800 * | 33 500<br>29 700 | 3 800* | 32.0<br>27.0 | 5 * |
| | | | | | | | Elong. in 5 inches | |
| 9/16″ | D | 12 | Max. 60 200<br>Min. 51 900 | 8 300* | 35 900<br>29 900 | 6 000* | 40.0<br>29.0 | 11 * |
| | G | 7 | Max. 61 300<br>Min. 57 700 | 3 600 | 38 500<br>33 200 | 5 300 | 38.0<br>30.0 | 8 |
| | | | | | | | Elong. in 4 inches | |
| 1/4″ | E | 4 | Max. 58 400<br>Min. 52 800 | 5 600 | 33 900<br>29 900 | 4 000 | 28.0<br>25.0 | 3 |
| | F | 5 | Max. 60 800<br>Min. 52 000 | 8 800* | 41 600<br>29 000 | 12 600* | 41.0<br>33.0 | 10 |
| | G | 32 | Max. 60 700<br>Min. 52 100 | 8 600 | 39 000<br>29 000 | 10 000 | 41.0<br>36.0 | 8 |
| | H | 8 | Max. 61 200<br>Min. 53 400 | 7 800 | 41 900<br>31 900 | 10 000 | 37.0<br>25.0 | 12 * |
| | J | 8 | Max. 76 000<br>Min. 63 700 | 12 300† | 36 900<br>33 300 | 3 600† | 28.0<br>22.0 | 5 † |

* Maximum for each gage.
† Maximum for ¼″ gage testing above 62 000 pounds.

TABLE B. (VARIATIONS FOUND IN THE SAME HEAT AND GAGE.)

| Gage | Heat. | Tensile Strength. | Heat. | Elastic Limit. | Heat. | Elongation |
|---|---|---|---|---|---|---|
| 3/8″ | L | 6 800 lbs. | L | 5 600 lbs. | K | 6.0% |
| 1/2″ | K | 6 500 " | A | 3 200 " | A | 4.0% |
| 7/16″ | R | 6 800 " | R | 3 800 " | R | 5.0% |
| 9/16″ | D | 8 300 " | D | 6 000 " | D | 11.0% |
| 1/4″* | F | 8 800 " | F | 12 600 " | H | 12.0% |
| 1/4″† | J | 12 300 " | J | 3 600 " | J | 5.0% |

* Tensile strength under 62 000 lbs.
† Tensile strength over 62 000 lbs.

Mr. Condron.

TABLE C. (VARIATIONS FOUND IN THE SAME HEAT AND GAGE.)

| Gage. | Heat. | No. of Tests. | Tensile Strength. | Max. Dif. | Yield Point. | Max. Dif. | Elong. 8″ —% | Max. Dif. % |
|---|---|---|---|---|---|---|---|---|
| 19-32″ | O | 14 | Max. 58 500<br>Min. 54 900 | 3 600 | 39 900<br>37 400 | 2 500 | 30.5<br>27.0 | 3.5 |
| 17-32″ | O | 4 | Max. 57 000<br>Min. 54 000 | 3 000 | 39 500<br>37 600 | 1 900 | 29.5<br>28.0 | 1.5 |
| 1-2″ | O | 4 | Max. 58 300<br>Min. 54 400 | 3 900 | 42 000<br>38 200 | 3 800 | 30.5<br>25.0 | 5.5 |
| 1-2″ | P | 10 | Max. 59 800<br>Min. 56 700 | 3 100 | 43 800<br>40 900 | 2 900 | 28.0<br>23.0 | 5.0 |
| 15-32″ | P | 32 | Max. 63 800<br>Min. 55 900 | 7 900 | 42 700<br>30 500 | 12 200 | 30.0<br>24.0 | 6.0 |
| 13-32″ | Q | 6 | Max. 61 600<br>Min. 55 800 | 5 800 | 42 100<br>38 600 | 3 500 | 30.0<br>26.0 | 4.0 |
| 3-8″ | R | 12 | Max. 66 700<br>Min. 58 200 | 8 500 | 44 300<br>40 700 | 3 600 | 27.5<br>22.5 | 5.0 |
| 5-16″ | S | 4 | Max. 61 000<br>Min. 59 700 | 1 300 | 41 300<br>39 800 | 1 500 | 26.0<br>25.0 | 1.0 |

differing gages of the same heat. Therefore, if only one test or even two tests per heat are required, it is important that such tests fall within narrow limits. If more tests are to be made, then it would be rational to expand the limits allowable for such tests.

In conclusion, the writer believes that the time has come for the recommending of a single uniform specification for structural steel for ordinary uses, and since our American manufacturers have demonstrated that they can make a steel of an average tensile strength of about 62 000 lbs., he would favor 62 000 lbs. as a basis, making the range four thousand pounds above or below 62 000 lbs. The recommendation of the committee of the Maintenance of Way Association has been for a range of 8 000 lbs., in which tests would be accepted, allowing the making of retests where the first tests are not more than 1 000 lbs. outside of the 8 000 lb. range. In the writer's opinion, this 1 000 lbs. should be made 2 000 lbs. The writer desires to suggest for your consideration the following range:

56 000 to 58 000 lbs. tensile strength . . . . 2 retests required.
58 000 to 66 000 lbs. tensile strength . . . . Tests accepted.
66 000 to 68 000 lbs. tensile strength . . . . 2 retests required.
Below 56 000 lbs. or above 68 000 lbs.
tensile strength . . . . . Rejected.

Of course the other properties would have to be fixed upon in harmony with this range and rivet and pin steel are not included. The former should be peculiarly soft and ductile, ranging between 50 000 and 58 000 lbs., tensile strength, and the latter quite hard like axle steel, ranging between 75 000 to 85 000 lbs. tensile strength. Mr. Condron.

THEODORE COOPER.—The evolution of the use of steel for structural purposes in this country may be said to have started about 1870, when Mr. William Butcher convinced Captain James B. Eads that he could furnish him steel of 100 000 lbs. tensile strength, able to stand without permanent set 60 000 lbs. compression and 40 000 lbs. tension. Mr. Cooper.

The metallurgical, mechanical and financial struggles of this experiment, while full of interest and instruction, were sufficient to discourage for some years further efforts in this line.

The next step of which the writer had personal experience was the "Hay" steel experiment of the Glasgow Bridge in 1878. This bridge was finally built of Acid Bessemer steel.

About 1887 Basic soft steel could be obtained on more favorable terms than iron of the same sections. From that time until now we have recognized two standard grades of structural steel. When, as was then true, they were really two different metals, viz: basic soft steel and acid bessemer medium steel—and, especially during the gradual disappearance of the metal iron and recognition of the new metal, the two grades were proper and necessary. Now practically these two grades are merely overlapping ranges of the same material. Under the Manufacturers' Specifications steel from 60 000 to 62 000 lbs. is both "soft" and "medium," and it is natural that where the orders for the two classes are equally probable, the objective point will be for material to suit either order.

Believing that both the interests of the steel manufacturers and of the steel users will be advanced by the adoption of *one common standard*, I think the time has come to advocate the same. But do not understand me to mean that we have yet got a material with which either the maker or user should or can be satisfied. But we can perhaps determine a common objective point or average material for a standard and the struggle of the immediate future, instead of being devoted to getting miscel-

Mr. Cooper. laneous varieties of steel to suit individual tastes or vagaries, should be to improve and perfect that one standard. With one average material in view the steel manufacturer should be able to systematize his output to give far greater uniformity than he has heretofore. The users of steel under *one standard material* should demand an important reduction in the limits of variation from the standard average. Under the present limits demanded by the manufacturers of steel there is a difference of 10 000 lbs. between the *minimum*, which determines the strength of our ships, bridges and other structures and the *maximum* which measures the workability of the material.

In spite of the manufacturers' limit of 10 000 lbs., there has been no difficulty for years in getting this limit reduced to 8 000 lbs.

Is it chimerical then to believe that with one class of material in view we can get this reduced to 6 000 lbs? If this result can be had, as I believe, why should we bend, form, work and punch steel of 70 000 lbs. to get a ship or bridge rated at 60 000 lbs? The difference in workability of 4 000 lbs. is worth money, and the manufacturer who can give the most uniform material should be favored even at an advanced price.

In advocating one standard structural material, I have in view all structures and not simply bridges. There is no just reason why the material for ships, bridges or buildings should be different, and there will be no gain in the character of our material, unless we can harmonize the requirements to suit all.

What is the average material which would meet the views and needs of shipbuilders, manufacturers of structural work, steel-makers and engineers, under the expectation of the future material being more uniform than we are now offered? I suppose it will lie somewhere between 60 000 and 66 000 lbs., many perhaps considering one too low and the other too high.

Personally I should consider, under the above claims, an average of 65 000 lbs. steel, or steel varying between the limits of 62 000 and 68 000 lbs., as the ideal material we should expect. The ductility and other qualities being at least equal to those now required of medium steel.

Mr. Greiner. J. E. GREINER.—There were a number of conservative engineers who made a strong fight for the retention of wrought

iron for structural purposes, and who kept up their struggle so long as iron could be obtained. Some of these engineers had a very unsatisfactory experience with steel structures and naturally preferred to adhere to a metal whose quality had been determined by long and continued use. Mr. Greiner.

A number of these engineers were of such standing as to give weight to their opinions. As a consequence the total replacing of rolled iron by rolled steel was a gradual process, although it lasted but a couple of years.

When it became very difficult to obtain a good wrought iron, this conservative element permitted the use of steel for certain parts of bridges, such as web plates in girders and eye bars in trusses, and finally, even when wrought iron was no longer obtainable it was still called for in specifications. The contractor in such cases was given the privilege of substituting a very soft steel, section for section.

In the meantime those who had entered the field of steel had made a sufficient number of tests to satisfy them that open-hearth steel varying from 52 000 to 65 000 pounds could stand more abuse than the best double rolled iron. It could be punched and sheared when thickness did not exceed 5-8″ with no greater injury. When the ultimate strength ran over about 68 000 pounds, punching and shearing had a tendency to injure the metal, but it was found that this damage could be removed by reaming and planing.

These conditions then naturally suggested two grades of steel, namely, a soft steel to be worked same as wrought iron, and a medium steel in which all holes are to be reamed or drilled and all sheared edges planed off.

It has been the general practice for the past six or eight years for bridge engineers to specify these two grades, the soft being used as a substitute for wrought iron, and medium for eye bars, or members which were to have holes reamed or drilled and edges planed, and to be used in structures of such magnitude that the saving in weight justified the more expensive shop work.

Engineers should keep pace with the times. The so-called soft steels served their time after they had quieted the nerves of the conservatives. That time has passed, and at present there is no material difference in the working qualities of the soft and

Mr. Greiner. medium steels as usually furnished and accepted. Either grade can be rolled from the same billet.

Some engineers have recently advocated the adoption of a soft steel limit between 52 000 and 58 000 pounds, and a medium grade between 62 000 and 68 000. It was desired to have a more decided distinction between the two, as well as to destroy the manufacturers' opportunity of supplying either from the same billet.

Such a classification is even more absurd than the present common practice. It would bar out what is really the best range for structural steel.

All things considered, an open-hearth steel ranging from 55 000 to 65 000 is about as satisfactory as can be obtained for structural purposes. It will meet all of the requirements for soft steel and will be about as strong as is usually accepted for medium. Such a steel can be worked under the same conditions as wrought iron, will be just as safe, and unit stresses may be taken 20% greater. This has been the writer's practice for the past two years, and is embodied in the B. & O. Railroad specifications for 1901.

A single grade as above outlined, will sooner or later become standard. If not recommended by some technical association of high standing, it is quite possible that the manufacturers will realize the advantages of such a single grade, not only to themselves, but to the users as well, and then we will be offered the choice of either accepting the standard product of their mills or paying a fancy price to gratify our own desires. The adoption of a single grade of open-hearth steel for structural purposes would not bar out the production of a special grade for use in very large structures where the dead load is a controlling factor in the design. The material for such structures, however, should be rolled in accordance with special specifications and not with general or standard ones.

Neither would the adoption of a single grade of structural steel affect the present soft steel output for rivets. We are not yet ready to use anything but a very soft steel for rivets, especially when driven by hand. When all riveting is done by power, there can be no objection to using the single grade steel as above outlined, even for rivets.

Mr. McLeod. JOHN MCLEOD.—I did not prepare a paper on this subject, because I felt that what I would have to say would have been

already said by those preceding me. I have listened with interest to the papers already read, and I would like to point out at this time that the manufacturers did not start this discussion. The suggestion came from the engineers. The manufacturers stand ready to furnish a soft, medium or hard steel, just as they have in the past. We think, however (this on the basis of our experience and from the benefit we have received from the experiences of others), that there is an intermediate grade of steel which will cover all requirements necessary for the average bridge structure, which, I think, would probably include at least 90% of the tonnage in steel. The other 10% would be the larger structures, for which special grades of steel might be required. The grade suggested by Mr. Cooper, I think, is toward the high limit, and therefore could not be used by the engineers who are particular to have holes reamed and edges planed for steel above a certain limit. I think, however, that a steel of between 55 000 and 65 000 lbs. tensile strength will not require such special treatment,—that it will be perfectly satisfactory to punch and shear such material without afterward planing or reaming. From a manufacturer's standpoint I would say that it would be of great benefit to the manufacturer to have such a specification adopted by the engineers, for the reason that it gives us an opportunity to have at our—what I would call jobbing—mills, the mills to which we send our blooms to be reheated and rolled into finished shapes and plates, a stock of material that we know would be of a certain quality and from which we could always draw; and we could thereby give better and prompter service to our customers and, through our customers, the bridge builders, we could benefit the engineers. I would therefore recommend not that we change our standard specifications (which I think we should not do, for the reason given before—that this is not now the American practice), but that we say to the American Engineering and Maintenance of Way Association that in our judgment there should be only one standard grade of steel for the average railroad bridge. I do not agree with Mr. Cooper that because that is a standard grade it is a cheap grade; it is, on the contrary, a little above an average grade; it is open-hearth steel, because we are now talking about railroad bridges, and I believe that they should be of the best steel that can be

Mr. McLeod.

Mr. McLeod. made. Therefore I do not think that Mr. Cooper is correct in his opinion that this steel should also be used for buildings. I think that in a structure such as a building free from impact, a material of not quite as high a grade as those suggested could be properly used.

Mr. Schneider. C. C. SCHNEIDER.—The writer desires to depart from the strict wording of the question and discuss the desirability of using one grade of steel for all ordinary structures, including material for ship building. If there is any advantage to the manufacturer in using a single grade of steel for railroad bridges, the advantage would be so much greater if the manufacturer of structural material could confine himself to that one grade entirely, excepting when special steel is required for structures of unusual magnitude, which occurs only once in several years.

This question: "Is it desirable to specify one grade of steel for all structural work?" may be answered by asking another question, viz: "Why do we use two grades of steel?"

The reason for this becomes apparent if we follow the history of bridge building in the country from the time when steel was first used as a structural material. In the earlier days steel was used only in structures of what was then considered unusual magnitude and only in certain members for the purpose of reducing the dead load and the sizes of the heaviest pieces, as at that time even the largest bridge shops were not fitted up for handling heavy work. To accomplish this, and also for reasons of economy, as steel was much more expensive than wrought iron and could be produced with a high ultimate strength without increasing the cost, a material of high ultimate strength and high elastic limit was specified and the working strains increased in proportion.

As engineers gained more knowledge of the properties of steel, it was recognized that this very high steel was not a safe material for railroad bridges, as it was not well adapted to resist impact. Engineers then commenced to specify more ductility and lower ultimate strength, and continued to lower their requirements for ultimate strength as the price of steel decreased until the usual practice had come down from over 100 000 lbs. to about 62 to 70 000 lbs., or what is now classed as medium steel. Table I gives the ultimate strengths specified for some of the more important bridges made of steel.

TABLE I.

Mr. Schneider.

| Year of Completion. | River. | Description. | Ult. Strength in Pounds per Sq. In. | Min. Elong. % |
|---|---|---|---|---|
| 1874 | Mississippi .. | Arches, St. Louis | 100 000 min. | 18 |
| 1880 | Missouri .... | Plattsmouth Bridge | 80 000 " | 12 |
| 1882 | Missouri .... | Bismarck ..... Comp. | 80–90 000 | 12 |
| | | Tension | 70–80 000 | 18 |
| 1882 | East River... | Brooklyn Bridge, excl. wire | 70 000 " | |
| 1883 | Niagara ..... | Cantilever | 80 000 " | 15 |
| 1884 | Susquehanna. | B. & O. R. R. ..... Comp. | 80 000 " | 15 |
| | | Tension | 70 000 " | 18 |
| 1885 | Arkansas .... | Van Buren Br ..... Comp. | 80 000 " | 15 |
| | | Tension | 70 000 " | 18 |
| 1885 | Ohio ..... | K'y.& Ind. Cantilever..... Comp. | 80 000 " | 15 |
| | | Tension | 70 000 " | 18 |
| 1886 | Harlem..... | Washington Arch, New York ..... | 62–70 000 | 18 |
| 1887 | Missouri .... | Sibley Bridge, At. & S. Fe .. Comp. | 75–85 000 | 18 |
| | | Tension | 60–70 000 | 23 |
| 1888 | Missouri .... | Omaha ..... Comp. | 80 000 min. | 15 |
| | | Tension | 70 000 " | 18 |
| 1888 | Mississippi .. | Cairo Bridge | 67–75 000 | 20 |
| 1888 | Ohio ..... | C. & O. R'y, Cin'ti & Coving'n Comp. | 64–72 000 | 19–17 |
| | | Tension | 58.5–66 500 | 20–18 |
| 1890 | Firth of Forth. | Cantilever, Scotland ..... Comp. | 76–83 000 | 17 |
| | | Tension | 67–74 000 | 20 |
| 1890 | Colorado .... | Red Rock Cantilever ..... Comp. | 64–72 000 | 19–17 |
| | | Tension | 58.5–66 500 | 20–18 |
| 1890 | Mississippi .. | Merchants' Bridge, St. Louis | 67–75 000 | 20 |
| 1891 | Ohio ..... | Cantilever H'y, Cincinnati | 62–70 000 | 22 |
| 1892 | Mississippi .. | Memphis Cantilever ... Main trusses | 69–78 500 | 18 |
| | | The rest | 64–72 500 | 22 |
| 1893 | Mississippi .. | Bellefontaine | 62–70 000 | 22 |
| 1895 | Delaware .... | P. R. R., Phila. ..... Main trusses | 62–70 000 | 22 |
| | | The rest | 50–60 000 | 26 |
| 1897 | Niagara ..... | Double Deck Bridge | 60–68 000 | 20 |
| 1898 | Niagara ..... | Highway Arch | 60–68 000 | 20 |

The *extensive* use of steel as a structural material did not commence until the year 1890. Before that time steel was used only in isolated cases or for heavier work such as chords and eyebars for larger bridges About 1890 some railroads commenced to build even smaller spans and plate girders of steel, and for eyebars steel was almost exclusively used. At about that time most of the structural mills that had formerly manufactured wrought iron had equipped themselves with steel furnaces, but continued for some time to make both kinds of material, until they found it more profitable to confine themselves to the manufacture of structural steel only and discontinued the manufacture of wrought iron. In 1894 it was practically impossible to obtain wrought iron shapes, and those engineers who would have preferred wrought iron were obliged to use steel instead, and naturally specified a grade of steel as

Mr. Schneider.

near wrought iron as possible, using also the same unit strains and workmanship as for wrought iron.

This accounts for the two grades of steel being at present in use—medium steel by the advocates of steel, soft steel by the advocates of wrought iron. As steel has now been extensively used for structural work for a long time—medium steel for at least twenty years and soft steel for at least ten years—we certainly have passed the experimental stage, and should be in a position to decide for *one* grade of steel for ordinary structural work. This will be of advantage to the producer and consumer. The manufacturer of steel will be able to conduct his business in a more systematic and rational manner than he can do at the present time. If he uses the same quality of stock he will be able to use the same mixture and obtain uniform results satisfactory to the specifications. The manufacturer of bridges and structural work will be able to keep the usual sizes of shapes and plates in stock, more particularly those of which there are only a few sizes rolled and which are very hard to obtain on short notice because the quantities used for each structure are small, such as fillers, stiffening angles, tie plates, lattice bars, connection angles, etc. This would therefore facilitate deliveries. The consumer will naturally also be benefited by the greater uniformity of the material which will result from only *one* grade of steel being manufactured.

There is still another reason why this Society should take a decided stand in favor of one grade of steel, and that is, that all other countries which have adopted standard specifications use only one grade of steel for structural work, and as the object of this Society is to establish a uniform practice all over the world, we cannot expect other countries to adopt our practice unless we have something practical and reasonable to offer which appeals alike to the manufacturer as well as the engineer. Crank specifications will certainly meet the opposition of the manufacturers abroad. Now admitting that it is desirable to use one grade of steel only, the question arises, What is the proper grade of steel for structural work? In deciding this question, we should consider the process of manufacture in its present state and the probabilities of the future. At present most of the structural steel is made by the basic process and

appearances indicate that the basic process will also be the process of the future for ordinary structural steel. It would therefore seem expedient to adopt a grade best suited to the basic process. For special higher grades of steel the acid process will probably hold its own. Mr. Schneider

The most satisfactory material which can be produced by the basic process averages about 58 to 60 000 lbs.; the writer would therefore recommend an ultimate strength of 60 000 lbs. per square inch, varying 5 000 lbs. either way. This grade of steel, in the writer's opinion, may be used without planing or reaming up to a thickness of about 5-8″ without impairing its strength. Table II gives the grades of steel used for bridges and structural work in some of the countries which have adopted standard specifications. After having agreed on

TABLE II.

SPECIFICATIONS FOR BRIDGES AND BUILDINGS.

| Authority. | Kind of Material. | Ult. Strength in Pounds per Sq. Inch. | Min. Elong. % |
|---|---|---|---|
| French Ministry of Public Works, August, 1891 | Plates and Bars at least | 60 000 | 22 |
| | Rivets at least | 54 000 | 28 |
| Austrian Ministry of Commerce, 1892 | Plates and Bars, lengthwise | 50–64 000 | 28–22 |
| | Transverse | 50–64 000 | 26–20 |
| | Rivets | 50–57 000 | 32–26 |
| Standard Specifications of German Societies, 1892 | Bars and Plates (of 1-4 to 1-1-8 in. thickness,) lengthwise | 53–63 000 | 20 |
| | Transverse | 51–64 000 | 17 |
| | Rivets and Screws | 51–60 000 | 22 |
| Federal Council of Switzerland, 1892 | Bars and Short Plates | 51–64 000 | 25–20 |
| | Long Plates, lengthwise | 51–64 000 | 25–20 |
| | Long Plates, transverse | 51–64 000 | 22–18 |
| | Rivets and Screws | 51–60 000 | 28–24 |

the proper grade of structural steel, the next question to decide is the quality. The quality is determined by the elongation, the bending test and the appearance of the fracture. As far as the elongation is concerned, we notice in the Specifications adopted by the Federal Council of Switzerland, and by the Austrian Government, that the specified elongation varies with the ultimate strength. The Swiss Specifications limit the product of the ultimate strength multiplied by the percentage of elongation in 8″ to not less than 1 280 000, and call this the coefficient of quality. The writer prepared Specifications for

Mr. Schneider. Railroad Bridges in 1886, which were published in 1887 as the Specifications of the Pencoyd Iron Works, wherein the quality of steel was specified as follows:

"Steel having an ultimate strength of 56 000 to 80 000 lbs. per square inch, when tested in samples of at least one-half square inch section cut from the finished product, will be accepted for the different parts of structures, provided it shows the following qualities:

"The elastic limit shall be at least 0.6 of the ultimate strength.

"The product of the ultimate strength per square inch in pounds multiplied by the percentage of elongation in 8" shall be at least 1 500 000."

This value was found by examining a great number of tests, of which only those were selected which proved, by the elastic limit, the elongation, the bending tests and the fracture, the good quality of the steel. A similar coefficient was also incorporated in the Bridge Builders' Specifications published about the same time.

As this appears to be a consistent and reasonable method to establish the quality of the material by varying the elongation with the ultimate strength, the writer begs leave to submit this question to this Society for consideration and discussion, and also the question whether the measuring of the elastic limit or yield point is of any practical value in establishing the quality of the steel.

Mr. Snow. J. P. SNOW.—As a buyer of bridges, I am in favor of a single grade of steel for ordinary structures; and I believe that from the point of view of the buyer, the builder, and the manufacturer of the steel, a single grade is desirable. It is not necessary, perhaps, to go into details as to the reasons why, because they have been so well brought out by others.

Although I am in favor of a single grade of steel, I would not be in favor of a single grade if it were of a high character. I would not be in favor of a grade as high as is recommended in Mr. Boller's paper. I prefer a steel having an average of 57 000 lbs. ultimate with a range of 4 000 lbs. each way; but for the sake of union I would not object to the grade that has been mentioned by two of the speakers here, that is, with an average

of 60 000 lbs. and a range of 5 000 lbs. each way. I am willing to admit this rather large range because I believe that if a single grade be generally adopted, it will be very easy in the future to reduce the range. As the mills become accustomed to turning out one grade the melters and other workmen in the mill will become expert in the manufacture of this grade and the product will be more uniform and all the melts will hit the average of the range much more closely than is now the case when the workmen are called upon to produce material all the way from extra soft to quite hard. I think there will be no trouble in the future in bringing the range down to 6 000 lbs., as suggested by Mr. Cooper. Mr. Snow.

I object to what is called medium steel—steel of 68 000 or 70 000 lbs. ultimate—for several reasons. Reaming and planing do not remove all the injuries which such steel receives in going through the ordinary bridge shop. It is absolutely necessary to straighten quite wide plates edgewise, say plates 24 or 30 inches wide and 3-8 inch thick. When such plates are straightened in a gag press, as is done in a good many shops, it takes heavy pressure to make them straight, and they must receive a very severe strain. I think that the point of the metal where the gag of the press acts must be in much the same condition as the side of a hole that has been punched, and of course reaming and planing do not remove this trouble. There are other considerations also; for instance, reaming is quite likely to take out only one side of the hole, material is likely to be partially heated and quickly and irregularly cooled, and so forth, so that reaming and planing which is generally specified in connection with medium steel does not, as is generally supposed, fully cure the ill effects of rough shop work.

The principal reason why I object to high steel is that I do not see any advantage in it. I can see that a rail, a splice-bar or axle is benefited by having rather high carbon, because they are subjected to very different action in service from what a bridge is. They are also treated differently in their manufacture: they are not punched or sheared, and they are not very seriously bent, although straightening a rail is a rather severe process; but the section of a rail is much better adapted for straightening cold than a thin, wide plate. Rails, splice-bars and axles are sub-

Mr. Snow.

jected to much more severe impact and greater reversal of stress than the members of a bridge, and hence should be made of material of higher elastic limit. Two considerations allow us to use higher steel in them than in bridges. First: their one method of failure is by breaking; deflection and vibration, which are so fatal to the reputation of a bridge, are factors of small importance with them. Second: their form is regular, without sudden variations in section, while the sections of bridge members must change abruptly where connections occur. Both of these considerations allow high unit strains in rails, splice-bars and axles, but not in bridges.

The reason, in my opinion, why a bridge made of 68 000-pound steel is not as good as one made of 58 000-pound steel is because it will of course be designed for higher unit stresses and consequently with lighter sections; and it will be a lighter bridge and will deflect and vibrate more under stress than a soft steel bridge will. The coefficient of elasticity is practically the same for soft and high steel, and that is what governs the deflection and vibration, and not the elastic limit or ultimate strength of the material. I believe that a bridge which acts light under traffic, which shows great deflection and vibration, will be condemned and taken out when the unit stresses under the loading become a certain fixed maximum which every bridge engineer has in his mind, although he does not always publish it. I believe such a bridge will be taken out whether it be made of soft steel or medium steel, or whether the holes be punched, reamed or drilled. I do not think that these factors make any difference whatever; the bridge will be condemned without reference to them. The fact is, a bridge is not loaded up to its ultimate strength; hence I think the ultimate strength of the material has nothing to do with the value of a bridge. Bridges very seldom break down on account of weak material. It takes a very poor bridge indeed to fail under its load. Still, great numbers of bridges are being constantly removed and replaced. The criterion for the condemnation of most bridges is their unsatisfactory action under traffic, which has but very little to do with the ultimate strength of the material of which they are constructed.

For these reasons, and for others which I shall not go into, I am in favor of what is called soft steel.

## GENERAL DISCUSSION.

W. R. WEBSTER.—There is one point to which I wish to refer. The subject has been treated as though it were just as easy to make a uniform basic open-hearth steel of 68 000 or 69 000 pounds ultimate strength as it is to make a 60 000 pounds grade. Now I would like to ask some of the manufacturers to give us their views as to what they consider the highest limit for such steel, to ensure as good uniformity as in the case of the lower grade. Mr Webster.

JOHN MCLEOD.—We consider the limits usually specified for soft and medium steel as within the limits of basic steel. We have always felt that we could make the steel uniform within those limits, and I think we have done so. We would prefer to make steel between the limits of 55 000 to 65 000; and that is what we are really doing. Most manufacturers make basic steel of about .10% carbon. It will test somewhere between 58 000 and 68 000 pounds; sometimes as high as 70 000 when the phosphorus is a little over .10%. Mr McLeod.

J. E. GREINER.—A statement was made by Mr. Condron, which is apparently in conflict with that just made by Mr. McLeod. Mr. Condron stated that from a canvass of steel manufacturers it appeared that they were all in favor of two grades of steel. Mr. McLeod says, if I understand him correctly, that the manufacturers would favor one grade of steel. That agrees with the results of my own canvass of manufacturers five or six years ago. They were all in favor of one grade of steel, if it could be adopted. Mr. Greiner

MR. CONDRON.—With regard to the canvass of this subject that was made by the committee of the American Railway Engineering and Maintenance of Way Association, it may be well to state that a large number of letters were addressed to members of that Association particularly interested in bridge work, and to a number of American members of the International Association for Testing Materials. About forty replies were received to something over one hundred letters that were sent out, and these replies were from seventeen chief engineers Mr Condron

Mr. Condron. or engineers of bridges of railroads; seven representatives of steel manufacturers; nine consulting engineers, and seven professors of technical schools. The replies from the steel manufacturers came from Mr. Albert Ladd Colby, of Bethlehem; Mr. Geo. E. Thackray, of Cambria; Mr. J. W. Anderson, of Carbon; Mr. A. Monell, of Carnegie; Mr. F. E. Abbott, of Illinois; Mr. James Christie, of Pencoyd, and Mr. H. H. Campbell, of Pennsylvania. The circular letter at first sent requested an expression of preference for ranges of ultimate strength and limits for other physical properties for soft, medium and rivet steel. As five of the railroad engineers replying to the letter suggested the use of a single grade of structural steel, rather than the use of soft and medium steel, a second circular letter was sent to those representatives of the manufacturers who had responded to the first letter, asking more specifically for their opinion of the advisability of recommending one grade of structural steel for bridge work. These gentlemen all replied to the second letter, stating that in their opinion, there was much to be said in favor of the use of one intermediate grade of steel, in the place of the so-called soft and medium grade, while in their answer to the first letter, they had naturally enough, all expressed preference for the ranges adopted for soft and medium steel by the Association of American Steel Manufacturers. If the circular letters that the committee sent out did not reach every member that was especially interested in the subject, it was certainly not intentional, as it was the desire of the committee to get as full expressions as possible from those who could assist in the solution of the specification problem.

Mr. Campbell. MR. CAMPBELL.—I am sorry that we cannot get up any very animated discussion on this; there does not seem to be any chance for it, but I think it needs an explanation. When the manufacturers are asked whether they want one or two grades, if they simply consulted their own business they would say one, but we do not want to take any such stand as that. When we are asked by an engineering body whether we recommend one or two, we must take into account the wants of our customers. We must speak as engineers; as men who are interested in the general public more or less, and in the engineering profession, and in the wants of our customers. We must take all those

grounds in our answer. If you ask us which we would rather make—one, two or five grades—we would say we would rather make one grade. As to whether the range should be 55 000 to 65 000 or 58 000 to 68 000, no man can answer that; no manufacturer has got it down quite as fine as that, and I don't think any engineer has, although he may say which he would prefer. If you want to agree on any one grade with any reasonable limits you would get the vote of every manufacturer. Taking all these things into consideration, I think that 55 000 to 65 000 would be the best limit. It is quite sure that in basic steel the higher you go, the more trouble you will have in getting uniformity. That is simply a mathematical proposition. It takes a certain amount of carbon to give you every additional increase in strength, and the more carbon you have, the more chance there is of lack of uniformity. When you get down to the narrow limit of 8 000 pounds you can pick out your tests and get it, but there will be great differences in the tensile strength of different thicknesses, sizes and shapes. If anyone thinks that he can get the same test out of a plate one inch thick that he will get out of a 3 x 3 x 3-8 angle, he doesn't know the fundamental principles. Mr. Campbell

George E. Thackray.—Regarding the question of one grade of open-hearth steel for railway bridges, it is undoubtedly true from a manufacturer's standpoint pure and simple, that it would be a much easier matter for them to make only the one quality; but on the other hand we are confronted by the differences of opinion of the customers. As an instance of this, please note the figures just given by Mr. Condron in which he explains that, in the first vote of forty people on this question, only three advocated the use of one grade, while the other thirty-seven thought that two grades should be maintained. If we take into consideration the differences of opinion of the numerous users and the variations in the method of manufacture and application, it is a question if the specifications should be changed to prescribe one grade only. Mr. Thackray.

The specifications as they now stand, including both soft and medium steel, are such that you can, if so disposed, select from them the grade wanted for the purpose intended, and by having this choice, the consumers are in a better position, as the

Mr. Thackray

differences of opinion which naturally exist among them are thus eliminated.

In regard to the remarks made by one of the speakers that the life of a bridge is determined by its vibration, it is known that steel of higher tensile strength will last longer and have greater endurance under reversals of stress, and this point has been particularly well demonstrated in the case of car axles in which it is found that the higher tensile strength material will endure much longer than the softer grades. If, however, it is merely a question of deflection within the elastic limit, that is referred to, the grade of steel is immaterial in this connection, for the reason that soft steel and hard steel have substantially the same modulus of elasticity and would deflect practically the same amount under equivalent conditions.

If again the question relates to the failure of bridge details, by reason of vibration, this depends largely upon the design, irrespective of the quality of the steel.

Mr. Condron.

T. L. CONDRON.—I believe it would be of interest to know what the manufacturers consider a safe upper limit for ultimate strength for structural steel that has to undergo the ordinary abuses in shop work and erection of bridges, and I would therefore suggest that the Chair invite the manufacturers present to give expressions of their opinions on this subject, especially with regard to basic open-hearth steel that is to be used either with or without reaming. I am an advocate of reaming of bridge material, and in my own practice, specify the reaming of all important members of a bridge. I do it, not simply to insure the removing of material injured by punching, but my observations in shops have satisfied me that reamed work is very much more satisfactory in every way. In my own specifications I have required that material should be punched with punches 3-16 of an inch smaller than the nominal size of the rivets used, and reamed to 1-16 of an inch larger diameter than the rivets to be used. This means increasing the diameter of the punched holes 3-16 of an inch on the die side, and ¼ of an inch on the punch side, under usual conditions in punching. As most of this reaming is done with the parts of the structure put together, it avoids the necessity of excessive drifting, and therefore permits the work being assembled without serious internal stresses being

set up. Such reaming as I refer to is a common practice in our western shops, and is generally specified by the engineers of our western roads. Mr. Condron.

JAMES CHRISTIE.—I think that is a question that cannot be definitely answered; but nevertheless in a broad, general way it is known that the higher the carbon is, the higher the tensile strength is, and the greater the liability of injury by disturbances a little more than superficial. Mr. Christie.

I heartily favor the suggestion of a uniform grade of steel for general structural purposes, and I think this should be material of from 55 000 to 65 000 pounds tensile strength, for the following reasons:

Basic open-hearth steel is rapidly superseding the acid process metal. The natural and usual product of the basic open-hearth furnace, working under its most favorable conditions, is steel of this tensile strength, with a tendency toward the lower limit. Steel of a higher tensile strength requires special treatment in the basic furnace, where the metal is usually refined in the presence of a highly basic slag, charged with oxides of iron. This metal finishes smoother and more readily than the harder grades. Furthermore, it is pretty well established now that for ordinary purposes little, if any advantage is gained by using the harder steel, inasmuch as the coefficient of elasticity of all grades is practically uniform. Higher working unit stresses are not generally desirable for the high as compared with the low tensile strength material.

J. P. SNOW.—We are told on high authority that as engineers we must not interfere with the method of making steel, because it is out of our province. By the same token manufacturers should not trouble themselves too much as to why bridges are condemned. They should take the engineer's word as to what makes a poor bridge. Certain replies have been spoken of—replies from manufacturers as to the proper grade of steel—and I well remember that one of them said that of course a bridge one part of which is made of 60 000-pound steel and another part of 70 000-pound steel is no better than a bridge made wholly of 60 000-pound steel, because the factor of safety, etc., is dependent on the lowest steel in the bridge. Mr. Snow

Now the value of a bridge depends on its action under

Mr. Snow. traffic. It doesn't make any difference whether it has an ultimate strength of 15 000 pounds to the inch more than a certain other bridge; we never get up to the ultimate strength; we keep far below the elastic limit, down to one-third of the ultimate strength; and the reason why bridges are condemned—and I do wish that the manufacturers would take the bridge engineer's word for it—is their unsatisfactory action under traffic. It is not dependent at all upon the ultimate strength of the material.

One other point of Mr. Campbell's I want to comment on: He admits that basic open-hearth steel with too much carbon is irregular. Now why cannot he tell us the carbon limit within which it is perfectly safe?

Mr. Campbell. MR. CAMPBELL.—It is a mathematical proposition that the more carbon you have in steel, the more irregularly that carbon will be distributed. I think that is a known fact. We can make a steel with .025% carbon and we can make a steel with 1.00% carbon, and as a general law the chance of irregularity will increase with the carbon content. But to say where it is safe, requires a definition of the word "safety." I profess perfect ignorance as to what is meant by that word "safe," and perfect ignorance as to where that limit is.

Mr. Webster. MR. WEBSTER.—I would like to ask Mr. Campbell whether acid open-hearth steel of 70 000 pounds tensile strength would not be more uniform than basic open-hearth steel of 70 000 pounds specified strength.

Mr. Campbell. MR. CAMPBELL.—I speak my personal opinion alone, and answer "yes."

The President THE PRESIDENT.—Does anyone wish to add to this discussion?

Mr. Campbell. MR. CAMPBELL.—I move that this question be referred to the American Branch of Committee No. 1, and that it is the sense of this meeting that a single grade of structural steel should be adopted. [Motion carried.]

# RAIL TEMPERATURES.

By Simon Strock Martin.

Ideas as to the actual temperature existing in a rail after leaving the finishing or final pass differ widely, and these ideas expressed in actual figures run from 750° C. to 1 100° C., according to the particular method used to determine the temperature.

The methods so far as known to me, are by the use of: (1) the pyrometric telescope; (2) the Le Chatelier pyrometer; (3) the water calorimeter.

First. *The pyrometric telescope* depends on the eye of the operator for results; hence it is influenced by the mind to such an extent, that different operators observing the same bar get very different results.

Recently one of the leading inspection bureaus of this country sent a representative to the Maryland Steel Company's mills to take readings on bars or rails after the final pass, and during four hours' observation the variation was only 8° C., showing either excellent work on the part of the Maryland Steel Company, or error on the part of the observer. Of the two alternatives, I prefer to accept the latter as the correct one. At any rate, this method gives only exterior temperatures.

Second. *The Le Chatelier pyrometer*, recognized by technical men as the only truly scientific pyrometer, has been applied to determining rail temperatures by placing the couple on a piece of the rail (cut just after leaving the final pass) near the centre of head, which is likely to give irregular results, on account of the unequal distribution of heat through the head of the rail. In order that the galvanometer may respond quickly, the couple is kept near the temperature of the rail, thus giving the needle only a short range. This method gives unreliable results on account of the application of the couple to different points.

Third. *The water calorimeter.*—This method consists in using the barrel calorimeter and noting the following data: (1) weight of water in calorimeter, (2) temperature of water before immersion, (3) temperature of water after immersion, (4) weight of rail end. Now instead of using the usual formula for lower

temperatures, and not knowing the specific heat for steel at high temperatures, we use the formulas given in *Annales de Chimie et de Physique*, Series 6, Vol. II, page 72 (1887).

The formulas for specific heat of pure iron are:

For 723° to 1 000° C.,

$$Q \text{ (given out to } 0°) = .218\ t - 39.$$

and for 1 000° C. to 1 158° C.,

$$Q \text{ (given out to } 0°) = .1989\ t - 23.44.$$

*Example: Crop weighs 22.5 lbs. and heats 104.5 lbs. of water from* 19° C. *to* 49.5°, *then,*

Total heat given out = 104.5 x 30.5 = 3187.2 Cal.

Total heat given out per lb. of steel = 3187.2 ÷ 22.5 = 141.6 Cal.

Specific heat of steel at about 60° C. = .118.

One lb. of steel, in falling from 49.5° C. to 0° C., gives out, 49.5 x .118 = 5.8 Cal.

Q (given out to 0°) = 141.6 + 5.8 = 147.4 Cal,

Whence 147.4 = 0.218 t — 39, or t = 855°.

Corrections must be made for heat lost to calorimeter during the time the temperature is rising, both by the water value of the calorimeter and the heat radiated. It is to be noted also that the formula is for pure iron, or iron free from carbon, so that it is necessary when the steel contains more than .25 C. to make allowance for same. Thus, for steel containing .50 C., its weight should be multiplied by 100.5.

This method gives excellent results, and to check their accuracy I used the following method: A Le Chatelier pyrometer was used for measuring the heat in a small furnace in which a rail end was heated. When the desired temperature was reached the rail end was taken out and placed in a calorimeter, and thus the two methods were found to check within 20° C., in a number of trials.

I find that a shrinkage of 5¾″ to 6¼″ corresponds to a temperature of between 780° C. and 860° C. I consider this a very accurate method for determining temperatures, and one easily applicable to mill practice. From the known accuracy of the Le Chatelier pyrometer, it seems that the last results are correct, and that all the heat of the rail has been measured,

and hence it shows us how far we are above the critical point in our present rolling.

Professor Sauveur's three micro-photographs showing .50 C. steel forged at known temperatures of 600° C., 850° C., and 1 100° C., give an excellent opportunity of determining by the microscope the temperature at which the rail left the final pass, and seem to me to be superior to any shrinkage, or other method that can be introduced for determining the final rolling temperature.

*Direct rolling temperatures as compared with reheating temperatures:* Mills rolling direct (of which there are a few in this country, and which are, I think, universal abroad) have advantages over reheating mills, and at the same time temperatures may be kept within a smaller range, because the ingots, if properly handled, in an up-to-date plant, are more uniformly heated than blooms.

When I speak of heating ingots which have been charged hot, I simply mean allowing them to "equalize" in the furnace, which might be properly called an "equalizing furnace."

It is known that mills built to roll direct are of heavier construction, with heavier rolls that will stand the harder work of colder rolling, and as the tonnage increases and the steel is forced through the mill faster—a condition arising in every mill—the proper procedure is to try to keep the product of a uniform structure, which can often be done. I know of one case in particular where the tonnage increased from 900 tons per 24 hours to 1 750 tons, and the structure was kept the same.

The structure of the material is better in direct rolling, because the bloom goes through the whole of the rail-rolls at a lower temperature, while in a reheating mill where the bloom has had a "wash heat" and is rolled at a high temperature to the final pass, and then held sufficiently to get the desired shrinkage, the result is a "case-hardened" rail, with a fine skin and a very granular interior which certainly is rolled at or under the critical point. This gives rise to stresses which, under impact, may produce fracture or failure, as shown by the micro-photographs of a certain rail in which the side of the head left the projected web, on account of the weak cleavage caused by the big faces of the crystals. This was on 85-pound, American

Society section rail which had been in the track three months. In rolling, this rail was held before the final pass so as to get the required shrinkage. It shows the importance of a homogeneous structure throughout the head of a rail.

I believe that it is important to reach as nearly as possible the critical point by continuous working from the ingot through the final pass.

Micro-photographs show that a reheated bar rolled very hot to the final pass and then held for a certain shrinkage has a more granular structure than a direct-rolled bar. I am of the opinion that rails can be rolled at the same temperature, no matter what their weight per yard may be, and that their structure will thus be the same.

It may be said further that in comparing a number of drop tests of rails rolled direct and others reheated and held before the final pass that the latter showed considerably more deflection. The Pennsylvania Railroad raised the specified maximum deflection last year to $2\frac{3}{4}''$, so that heats might not be rejected that showed such tendencies the year before. Evidently there must be some reason for this difference in deflection, and as both rails had the same chemical composition, it would seem that the direct-rolled rail has a higher elastic limit. This condition will probably be explained by Professor Sauveur's description of the result obtained by holding before the final pass.

Summing up, it would seem that shrinkage is certainly not an index of structure, and the only way to determine structure is through the microscopic examination of the product of individual mills. Some typical micro-photographs are appended.

NOTE:—The joint discussion of this paper and the succeeding one on "Structure and Finishing Temperature of Steel Rails," by Albert Sauveur, follows on pp. 85–96.

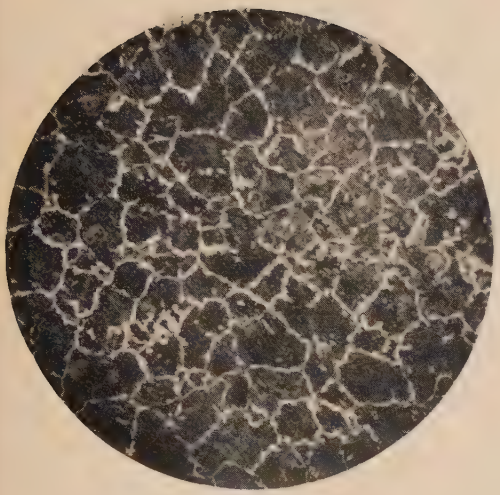


FIG. 1.

From center of head of 100-lb. rail rolled direct at low temperature throughout the rail rolls. Shrinkage: 7 in. in 33 feet. Magnified 45 diam.

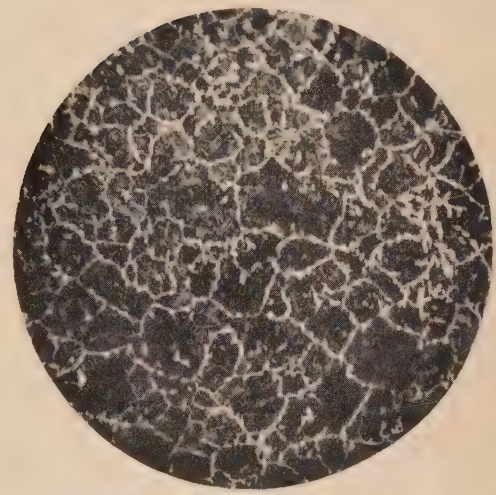


FIG. 2.

From center of head of 100-lb. rail rolled direct, but at a higher temperature than Fig. 1, and held before finishing pass until cold enough to give 6½ in. shrinkage in 33 feet. Structure larger than in Fig. 1. Magnified 45 diam.

FIG. 3.

From center of head of 100-lb. rail rolled by usual process from reheated bloom. Shrinkage: 7⅜ in. in 33 feet. Magnified 45 diam.

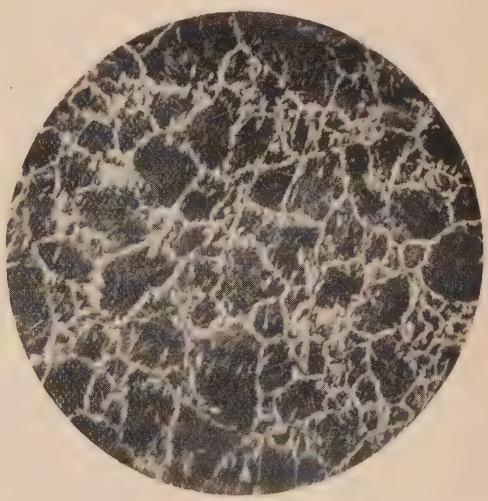


FIG. 4.

From center of head of 100-lb. rail rolled by Morrison-Kennedy process from reheated bloom, held before finishing pass. Shrinkage, 6 in. in 33 feet. Magnified 45 diam.

# STRUCTURE AND FINISHING TEMPERATURE OF STEEL RAILS.

By Albert Sauveur.

It is well known that when a piece of steel is allowed to cool undisturbedly from a high temperature, it crystallizes, and that the resulting crystals or grains, as they are frequently called, are the larger, the higher the initial temperature and the slower the cooling. This was first forcibly stated by Professor Tschernoff in his masterly paper on "The Manufacture of Steel and the Mode of working it," communicated to the Russian Technical Society in April and May, 1868.

It is also well known that if the steel be vigorously worked (rolled or forged) while it is cooling from a high temperature, crystallization is prevented, but as soon as work ceases, crystallization sets in until a certain temperature is reached which in the majority of cases is not far from 700° C., and below which there is no further growth of crystals. If work be performed below that critical temperature the crystals are permanently distorted, being flattened and elongated in the direction of the rolling and this distortion of the crystals is greater the lower the temperature at which the work is carried on. Such distortion causes a decrease of ductility, and eventually, brittleness, a striking illustration of which is found in the drawing of wire, which must be frequently annealed to remove the brittleness produced by work at a low temperature. The annealing removes the distortion of the crystals and, therefore, the resulting brittleness by causing the metal to assume a new and finer crystallization.

We may, for convenience, call the range of temperature during which steel crystallizes, and which extends from the melting point to the critical temperature (about 700° C. in the case of carbon steel), the crystallizing range, and the work performed at these temperatures *hot work*, while the work done below these temperatures will be called *cold work*.

Hot work influences the final structure by preventing or retarding crystallization, while cold work distorts the crystals.

It follows from the above considerations that in working steel, as is done in the manufacture of so many implements, from a high to a much lower temperature, crystallization is retarded, *i. e.*, is made up to cover a much shorter range of temperature, extending from the *finishing temperature* (*i. e.*, the temperature at which work ceases) to the critical temperature. The resulting structure will, therefore, be finer grained, *i. e.*, will be made up of smaller crystals, than if the metal had been allowed to cool *undisturbedly* from a high temperature: the crystals will be the smaller the lower the finishing temperature. Should work be continued below the critical temperature the crystals will be distorted, which implies a loss of ductility.

It has been conclusively shown that the finer the structure, *i. e.*, the smaller the crystals, the more ductile will be the steel, and since ductility is always a very desirable property, whatever the intended use of the finished implement, we should so conduct our treatment of the metal as to confer upon it the finest possible structure. The importance of finishing steel implements at the proper temperature, therefore, need not be insisted upon. It is now appreciated by all enlightened metallurgists.

The manufacturers of steel rails have, more than any other producers of finished steel articles, given careful attention to the important influence of the finishing temperature upon the structure and the physical properties of their rails. In these days when the tendency is to allow more and more carbon in rail steel, in order to lengthen the life of the rail, the importance of securing all the ductility possible from the heat treatment stands pre-eminently at the front.

The importance of low finishing temperatures was clearly brought out some ten years ago at the South Chicago works of the Illinois Steel Company, through some extensive tests conducted by the writer, the results of which were summed up in a paper presented at the Engineering Congress in 1893. In spite of the conclusiveness of these tests, however, the management could not be made to take up any efficient measure toward lowering the generally too high finishing temperature of the rails. It was assumed that any step in that direction would interfere with the output and this was not to be considered. While the results given in the paper did not attract much attention at the time, a

seed had been sown that was destined to bear much fruit. The importance of proper finishing temperatures gradually asserted itself until at the present day there is hardly a steel rail user or producer who is not fully alive to it. The results of many experiments along this line have been published within the last twelve months, which, although they merely confirm the conclusions reached some ten years ago, are, curiously enough, treated by their authors as newly discovered facts. This peculiar attitude on the part of writers who ought to be familiar with the previous treatment of their subject, attracted the attention of the *Railroad Gazette*, which says editorially, under date of May 31, 1901:

> In all the discussions on the heat treatment of steel the matter has been treated as though it was new, and the writers appear to have forgotten the early work of Tschernoff, Metcalf and others, and the later work in the early 90's. It is well to realize thoroughly that this is no new fad. Notwithstanding all that has been said on this subject it has been a hard matter to get engineers to realize that steel can be rendered almost worthless by being finished too hot in rolling When this important fact is fully appreciated we may expect to get the best results from steel, as means will be taken to give it the proper heat treatment in rolling.
>
> In Mr. Metcalf's classical paper on steel, presented before the American Society of Civil Engineers, March 2, 1887, and reprinted in the *Railroad Gazette* of March 18, he says: "As steel congeals it forms crystals and the sizes are largely affected by the rate of cooling; slow cooling favoring the formation of large crystals, quick cooling and also agitation producing fine crystals. . . . In every piece of steel that is in existence to-day there is a sure record of the last temperature to which it was subjected as well as of the manner in which the steel was worked. I mean to say that for every variation of heat that is visible to the naked eye there is a corresponding variation in structure which is equally visible to the naked eye if the record be opened by fracturing the piece. . . . For a double quantity of carbon we have five times the difference in specific gravity, due to an equal difference in temperature. . . . The finest crystals and the best structure can only be formed by quick cooling and the violent agitation of the hammer or of the rolls."
>
> At the World's Fair Congress of Engineers, in Chicago, Mr. Albert Sauveur, then in the employ of the Illinois Steel Company, gave the results of his investigation on the heat treatment of steel rails in six propositions, which are well worth the study of any one interested in this subject. His paper will be found in the *Transactions* of the Institute of Mining Engineers, Vol. XXII, from which we quote:
>
> "A polished and etched section of steel rail when examined under the microscope does not by any means reveal in all its parts the same struc-

ture. This heterogeneousness of structure is due to the different temperatures at which the various parts of the rail leave the finishing-rolls, and to the unequal rate of their subsequent cooling. The propositions formulated in the first part of this paper should enable us to foretell such variations in the structure of a rail.

"The outside of a rail, leaving the rolls coldest and cooling quickest, will offer less chance for crystallization, and should therefore show a smaller grain than the inside, which, being the hottest and cooling down more slowly, will favor the crystallization of the metal, the size of the crystals or grains reaching its maximum in the centre of the head. It might also be inferred that the smallest grain will be found at the extremities of the flange, since this is always the coldest-finished region. Proceeding from the outside to the inside we shall find the tendency toward coarse crystallization gradually increasing."

*The Kennedy-Morrison Rail Finishing Process.*—The suddenly renewed interest in the subject of proper finishing temperature for steel rails, just alluded to, led the Carnegie Steel Company to a notable departure at their Edgar Thomson plant, from the ordinary mode of proceeding. This move on their part was given much publicity and has attracted the attention of all rail consumers. Stated briefly, the modification which they have introduced in the rolling of steel rails consists in holding the rails on a cooling table a few minutes before subjecting them to the last pass. It is argued that by so doing the rail is finished at a lower temperature, which is the desideratum aimed at.

Let us look critically into this new departure in rail rolling. The rail when it reaches the cooling table introduced before the final pass is at a high, or relatively high, temperature and is then allowed to cool until a certain lower temperature is attained. During this undisturbed cooling the metal evidently crystallizes and the rail must necessarily enter the finishing rolls in a crystalline condition, which will be the more pronounced the higher the temperature of the rail when it reached the cooling table and the slower and more prolonged the subsequent *undisturbed* cooling. It also seems as if the single pass to which the metal is subjected after this crystallizing period cannot be sufficient to break up the crystallization just created. On theoretical grounds, therefore, if a material gain results from this method of proceeding, I do not see how it can be accounted for. Let us remember that any *undisturbed* cooling above the critical temperature constitutes a crystallizing period. By partially cooling the rail before the

final pass, we shorten the crystallizing range after the rail has left the finishing rolls, but we create a crystallizing period just before the final pass, which is exactly equal in length to the diminution of the final undisturbed cooling. We do not, by so doing, decrease the *total* crystallizing range, which alone governs the size of the final crystals; we merely divide it, causing a part of it to be located immediately before the final pass and the balance immediately after, so that a rail treated by this process has passed through a crystallizing period of *exactly the same length.*

The only beneficial effect, therefore, that can be claimed for this process is that it breaks up the crystals produced just before the last pass, resulting in a finer structure rail. If we consider that the reduction produced by the last pass is about 5%, it seems hardly conceivable that so slight an amount of work could be effective in breaking a pre-existing coarse structure.

Similar criticisms of the value of the Kennedy-Morrison process have been published by Mr. S. S. Martin, of the Maryland Steel Company, who contends, apparently on good ground, that any undisturbed cooling introduced in the process of rolling, in order to be effective in producing a finer grained rail, must take place at an early period, because then the considerable reduction, *i. e.*, the great amount of work to which the rail is subsequently subjected, will break up the crystals formed during that undisturbed cooling. It has also been argued, and rightly, I think, that those mills which do not reheat their blooms produce finer grained rails, for the simple reason that they finish their rails at a lower temperature.

To sum up, in order to confer a fine-grained structure upon steel rails, three courses seem to be opened:

1. To shorten the crystallizing period, *i. e.*, the time during which the rail is allowed to cool undisturbedly above the critical temperature (say about 700° C.). In other words, the rail should be finished sufficiently cooled, and the finishing temperature will, of course, depend in turn upon the temperature of the ingot when rolling begins and upon the speed of the operations. The main objection to starting and carrying the rolling at a relatively low temperature is that it throws more work upon the rolls. The objection to a slow rolling speed and handling of the material is

that it decreases the output of the mill. Both objections imply an increase in the cost of the rail.

2. To cause a part of the crystallizing period to occur previous to the final pass, *i. e.*, to allow the rail to cool undisturbedly at some stage of the operations, and for a short time, before completing the rolling. This cooling in order to be effective should occur at a sufficiently early stage of the rolling, in order that the resulting crystallization may be broken up through a sufficient amount of subsequent work and reduction. Such results would be obtained by placing the cooling table at the shears. The objection to this course is that the shaping of this cooler bloom into rails would require greater mechanical effort and, therefore, increase the cost of production.

3. To finish the rail at the temperature most desirable for easiness and speed of manipulation in rolling and then to reheat it to a temperature slightly above the critical temperature, a treatment which would result in the breaking up of the pre-existing coarse structure, replacing it by a much finer one. This course would undoubtedly be the most effective of the three as it would not only impart a fine structure to the rail, but also a *uniform* structure to all its parts, while a rail not so treated must necessarily be coarser in the central parts than nearer the cooling surfaces, owing to the higher temperature of the centre when the rail leaves the finishing rolls and to its slower subsequent cooling. The cost of such reheating treatment would undoubtedly be very great and, possibly, prohibitive.

# DISCUSSION.*

P. H. DUDLEY.—Professor Sauveur in his valuable paper, by treating the subject as a general proposition, without mentioning any of the relations a rail sustains by its section and stiffness to railway service and interests, perhaps condenses his paper too much, for it will be read rather in an educational than a technical sense by a majority of readers. Mr. Dudley.

Many will infer that colder rolling is the only important requisite now required to make rails which will meet the demands of present service, reducing the loss of metal by wear to as slow a rate as was the case on the early Bessemer steel rails of two decades ago under the lighter wheel loads. This would be a return to the practice which then prevailed with the limited output, and a price of rails of three to four times the present cost.

Besides a return to the slower and consequently colder rolling from the small ingots of the early Bessemer steel rails which replaced iron, a sound ingot is needed of a grade of steel which will produce the essential physical properties for the rails as girders and also insure a slow rate of wear.

It should not be forgotten that while we are all familiar with the rapid doubling and trebling of the wheel loads in the past decade, the stresses in the rails as girders are principally met by elasticity of length, while the increased intensities of the wheel pressures per unit of contact between the wheel treads and the rail heads are sustained more by the elasticity of volume, which is quickly reduced by the slightest unsoundness of the metal in the surface of the rail head. Therefore, to have the most benefit from colder rolling or heat treatment the metal must be sound and homogeneous.

The evolution of the present "up-to-date steel mill" has been more in the direction of increasing the "output" and decreasing the cost of the product than of any intention of decreasing the quality of the finished rails.

The mechanical conditions for an increased output have

* Joint discussion of the two preceding papers, viz: "Rail Temperatures," by Simon S. Martin, and "Structure and Finishing Temperature of Steel Rails," by Albert Sauveur.

Mr. Dudley.

received most of the consideration. It was not anticipated from a metallurgical standpoint—the latter not being so well understood—that there would be a decided reduction in the quality of the product from the same composition as was used for the earlier sections. These had more work, in fact, by eliminating the essential time element for crystallization in the slower and colder rolling. Now that a large product can be secured, the question of quality for present service is being considered more generally.

There does not seem to be any reason why the construction of a rail mill is not possible which will permit not only much colder rolling and finishing temperatures than are now general, without restriction of the "output," but turn out a more uniform and better finished section than is now produced. The standards of smoothness of track are now much higher than a decade ago on all of the important trunk lines, and they have not reached their limit. The smoothness of the surface of the rails rather than its wear now determines when they must be replaced.

Professor Sauveur states several facts—principles—which he says are old and well known, particularly the one in the first paragraph, stated by Professor Tschernoff in April and May, 1868. This was the period of active substitution of Bessemer steel rails in this country, some of the first having been put into service in 1863. Crucible steel had been tried in a limited way for the purpose of securing a fine-grained and homogeneous metal for wear.

Booth's crucible steel capped rail head was tried extensively to secure a portion of the head of fine-grained material for wear, the balance of the section being of iron. Both practically and technically it may be said to have been recognized by many people for a long time that for the slowest rate of wear in the heads of rails the metal should be fine grained. Experience has confirmed this and also the important fact that the metal must be sound and homogeneous.

While such facts may be known but not generally understood, it is difficult to have them incorporated as essential features in the manufacture of rails.

Technical principles of far-reaching importance must be

stated, absorbed and digested by hundreds of minds before they can be reduced to practice in a commercial sense for railway interests. This requires years of discussion in technical societies, and circulation of their papers. Mr. Dudley.

In the first paragraph and the first sentence of the second of Professor Sauveur's paper, he states, as he says, well-known facts. The statements are generally true as applied to blooms or material which has been rolled from the ingot, but do not cover all the work to be done when rolling rails direct from the ingots, or to blooms.

The fluid steel of about 1 450° C. is tapped from the teeming ladle into the mold, the steel on congealing forms a decided columnar structure interiorly from the sides of the iron molds of one to one and a half inches in thickness, the interior mass crystallizing in the usual granular or polyhedral forms. The ingots after cooling fifteen to twenty minutes are set sufficiently to be stripped, and are then placed in furnaces or soaking pits until the heat of the interior and exterior portions of the ingot is sufficiently equalized at about 1 110° C. or 1 150° C. for blooming. The work of the blooming passes breaks up the coarse columnar structure though rarely without several skin cracks occurring, which, though not completely, roll out partially in the roughing and finishing passes for the rails. Some "seconds" are produced, the percentage being high when exceeding five or six per cent.

It requires close attention to the composition and manufacture to produce an ingot with a columnar structure that will withstand the subsequent heating and blooming without cracking. "Cobbles" are not infrequent. If the rolling is direct from the ingot to the finished rail, then the work of the passes must break up not only the ingot structure, which seems to be done before or during the "dummy" pass, but prevent the building up of a coarse structure until the work ceases. This should be near or slightly below the critical temperature for the given composition.

Rolling or forging not only can break up a structure during "hot work" but check a large crystallization to the critical temperature for the steel.

"Hot work" influences the final texture of steel by break-

Mr. Dudley.

ing up the ingot or bloom structure, as the case may be, reducing the steel to a plastic or possibly a porcelanic condition, and by eliminating the essential time element for the crystalline forces to rearrange the combinations retards crystallization from a higher to the critical temperature for the composition.

This important principle, so clearly set forth in other words in paragraph 2, page 2, of Professor Sauveur's paper, is the key-note of practice to be observed to improve the quality of steel rails during the period of PROPER HOT WORK. It can be so well applied in practice as not to restrict the output.*

"Blue working" is not advisable though below this temperature the steel again increases in tensile strength, then reducing slightly as it reaches 30° C.

Some of the early foreign Bessemer steel rails were rolled until the critical temperature in the head was passed, while one firm toughened their rails. In regard to ductility, I place more restriction upon it than Professor Sauveur does in his paper. In rails I insist upon high elastic limits in the metal and then the requisite ductility for safety and good wear of the rail heads.

Rails in the tracks of steam railways to carry the moving wheel loads must perform the functions of continuous girders, and we are limited as to the amount of ductility we may give the metal for rails.

The rails must be elastic and should have high elastic limits, 55 000 to 60 000 pounds, and then have the requisite ductility and fine-grained structure in the metal for wear.

Unit fibre stresses in the base of rails under high speed trains, even in 80 lb. rails of 45 000 lbs. in tension, are not uncommon.

The introduction of stiffer rails as girders in the tracks and the higher standard of maintenance possible have permitted the doubling of the driving wheel axle loads, the tractive power of the locomotives, the passenger train loads and increased the freight train load four to six times during the past decade.

The steam pressures have been increased from 135 to 205 lbs. due to the use of steel in locomotive boilers and the latter

* Subsequent to writing this discussion, some rails were rolled so cold, though not to or below the critical temperature, as to produce a non-granular structure. The elastic limits, however, were reduced below what was desirable. The effects of colder rolling do not seem to follow the same laws as thermal treatment and there is much room for further investigation.

increased in capacity, so they can generate and maintain an expenditure of 1 200 to 1 500 horse power at sixty and sixty-five miles per hour. Such service is only possible on the stiffer and heavier rails of the present. Mr. Dudley.

It is a fact now known but not generally understood, that the stresses in the rails increase decidedly under the large expenditures of tractive power, beside that due to the weight and speed of the locomotive. With the increased service which has followed the introduction of the heavier and stiffer rails there has been a legitimate increased wear of the head, while there has been some increased wear due to the quality of the product. The increased axle and train loads have resulted in a decrease of operating and maintenance expenses, so that from the financial standpoint it is found desirable to use the stiffer and heavier rails. The demand for such rails at the present time (1902) exceeds the output of the mills, and any manipulation of the process of manufacture which will limit the output is not likely to receive much consideration. There is no reason, however, why the output need be limited to insure much colder rolling or the manufacture of a fine-grained rail.

Rails rolled direct from the ingot have a finer texture, as would be expected, than those from the same composition rolled from re-heated blooms. The shrinkage allowances now prescribed for 80 and 100 lb. rails are yet in excess of the allowance for the former lighter rails, and some reduction in the future may be expected. Professor Sauveur has summarized the steps which may be followed to secure colder rolling or heat treatment.

ROBERT JOB.—Professor Sauveur has given a clear and concise summary of present knowledge regarding influences of structure upon physical properties of steel, much of it being derived from his own published work in and since the important paper of 1893.* The latter, with the work of others as a basis, placed the subject of heat treatment of steel in a definite and practical form, and great improvement in quality has resulted. Mr. Job.

In connection with the term "finishing temperature," we often find a good deal of apparent misconception, and it seems

*Transactions American Institute of Mining Engineers, Vol. XXII, p. 547.

Mr. Job. desirable, at the risk of repetition, to state that the low finishing temperature desired is a temperature near the critical point at the time when thorough working of the steel ceases. To secure the best results, as stated, it does not appear sufficient to carry on 95% of the reduction at a relatively high temperature, and only 5% at the lower temperature, and it has been shown by Mr. S. S. Martin, and also by results presented a month ago at the joint meeting of the Franklin Institute and of the American Institute of Mining Engineers at Philadelphia, that such procedure toughens the steel merely to the depth to which working extends during such last reduction, and that the condition at centre of head is practically unchanged.

As an indication of the attention which is being given to the study of heat treatment, the discussion before the Iron and Steel Institute, London, last month is noteworthy, and the statement by Sir Lowthian Bell that a solution to the cause of brittleness in certain rails under the drop test would probably be found by following out these lines of study, is of especial interest in view of his well-known experience that rail failures have been seldom explained by the ordinary chemical analysis.

The whole subject of manufactured rails is passing through a period of transition, and we feel certain that by practical study and experiment along the lines which have been worked out, methods will be perfected which will result in the fine-grained, tough, ductile quality which gives the greatest durability in service, and this without decrease of tonnage at the mills, or increase in cost of production.

Mr. Howe. HENRY M. HOWE.—I have been asked to tell you what we are about doing at Columbia University to throw light on the relation between the finishing temperature of steel and its microstructure and physical properties. We have put in a small roll train driven by a motor, and arranged a furnace for extremely accurate heating. What we propose to do first of all is to heat bars of steel all exactly alike to the same given high temperature and allow them to cool: we shall let one cool to 1 300° C. and roll it through one pass instantly, and then let it cool completely; another we shall allow to cool to perhaps 1 275° and roll that instantly; another to 1 250° and roll that instantly, and so on. We shall finish a lot of bars of steel all exactly alike, in exactly

the same way, except that the finishing temperature will vary. We should know within ten degrees what the finishing temperature is. We shall then determine the physical properties, and the microscopic structure. It seems to me that in that way we shall be able to make a start towards getting some absolute, definite, quantitative knowledge on this subject. We shall know for this particular steel what microstructure corresponds both to a given finishing temperature and to given physical properties. Given the microstructure, we can tell at what temperature this steel has been finished and also what its properties are. With this we shall make a start, and I think in the right direction. Mr. Howe.

THE PRESIDENT.—It should be remembered that it was decided to take up in connection with this discussion the question of the desirability of introducing into our standard specifications for steel rails a clause concerning heat treatment.* The President.

JOHN MCLEOD.—I have followed with interest the papers of Mr. Sauveur and Mr. Martin, and I would like to say that our company has undertaken some improvements in the heat treatment of rails. Before doing that we were confronted with the desire of the engineers to introduce high carbon requirements into their specifications in order to overcome the hot finish and in that way try to get rails which would endure as well as the old rails, which were very much lower in carbon. In a discussion which took place five or six years ago in the Chicago office of R. W. Hunt & Co., I asked Captain Hunt: "Do you agree with me that if, with .35 to .40% carbon, we produce the structure in an 80-pound rail that exists in a 60-pound rail that the 80-pound rail will be as good as the 60-pound rail?" He replied: "Certainly, I agree with that." I said: "Very well, then, we had better take this question up along those lines and see what can be done." There were present at that meeting Mr. Morrison, of the Morrison-Kennedy process; Mr. Thomas Johnson of the Pan Handle; Mr. C. L. Taylor, of our company; Mr. Pearce, of the Chesapeake & Ohio, and several others. Thereafter when Mr. Morrison started on this scheme, aiming to get the same tonnage and yet improve the structure of the rail, he was confronted with the same theory which Mr. Sauveur has Mr. McLeod.

*See p. 49.

Mr. McLeod. explained as his theory of the proper manipulation of steel, that the process should extend farther back than just before the last pass. It is a very natural theory; but after our experiments we could not find that we derived any added benefit. This experience simply confirmed what we had already convinced ourselves of in connection with the finishing temperature of eye-bars, where we had tried the next to the last, the second and the third from the last pass with little, if any, added benefit. It would not have been an impossible thing to have gone farther back than the last pass in our rail mill; it would simply have meant pushing two roll-trains over instead of one and having the starting point further back; but since we felt that it would be useless, we decided not to do it. Before changing our mill, however, at considerable expense to our company, we rolled some rails by holding them before the last pass and put them in the track at a point in our yard where we had always rejected rails after a service of a little less than a year. After a year had passed the inspection of those rails showed that they were good for at least another year's service—that the capacity of the rail had been doubled. We then decided to change our mill as it has been changed under the Morrison-Kennedy process.

Now, Mr. Sauveur may be right, for I do not think that we have gone far enough to say that he is wrong; and his theory certainly seems logical, but I do believe that as far as we have gone the rails we have made under the Morrison-Kennedy process will develop a much longer life than the rails that we rolled under the old practice.

Other rail mills may accomplish in another way what we are attempting to accomplish in our way, and their rails may be just as good or better than those we are turning out. But I believe that we should wait for more definite information before undertaking to change our specifications. However, we, as a company, think that the shrinkage test is a convenient method of checking the finishing temperature, provided due allowance be made for the time element between the last pass and the hot-saws, since the distance from the rail-train to the saws may be quite different in different mills.

The President THE PRESIDENT.—I should like to bring out one more point. Mr. McLeod just said that the test of rails in their yard showed

double endurance where the rails were finished after being held for the last pass. I should like to ask what was the cause that took the rails out of service under the old practice, and what was the cause that took them out of service with the modified practice. The President.

MR. MCLEOD.—I am sorry I cannot enlighten you on the latter part of your question. The rails were simply worn out in the first instance. You may be sure they are worn out when a manufacturer discards them himself. The rails made by the new process, after having given service which would have caused the death of rails made by the old system of rolling, were in such condition that they seemed to be good for another year. I am sorry that I have not the information which would enable me to make a definite statement as to how long they did last. Mr. McLeod.

THE PRESIDENT.—The answer Mr. McLeod gives enables me to bring out the point that I had in mind, namely: when is a rail worn out? Some years ago some of you may remember we ourselves made a little study of steel rails; and in the course of that study the point was developed that when a rail has worn down from the top so that it is three-eighths inch less in height than it was when it was put in the track, that rail is no longer fit for service in main track; not because the rail can be exactly said to be worn out, but because the top of the rail has become what we call wavy or undulating. In this condition it produces rough riding in the cars. Such rails are usually drawn from the main track and put either in sidings or on branch roads. The President.

Professor Sauveur indicates that the 5% reduction produced at the last pass does not probably affect the steel clear through. Does it affect steel three-eighths of an inch; and if so, haven't you got the real valuable wearing structure of the rail what it ought to be?

E. F. KENNEY.—I do not understand exactly the ground that Mr. McLeod takes in objecting to this information being put in the specifications. If we do not specify a colder treatment for rails, how are we engineers going to get rails finished that way, and if we do not get them, how are we to obtain any information as to their wear? It seems to me that it would be advisable to try this plan to find what the wear of rails finished colder actually is. Mr. Kenney

Mr. McLeod. MR. MCLEOD.—We are perfectly willing that you should put that in your specifications, in so far as we are concerned, but the shrinkage that would suit the conditions at our mill might not suit another mill in which the distance from the rail-train to the hot-saws is entirely different.

Mr. Campbell. H. H. CAMPBELL.—Our saws are about a foot and a half away. We would like to get them a hundred feet away, but it would cost us a hundred thousand dollars to do it; as a matter of fact, we roll two lengths, and there is just room to saw the ends off and that is all. In other words, the mill was built for a 30-foot rail. I recognize that no manufacturer has any right to ask concessions on account of any antiquated condition of his mill, but I am not asking any concession in the quality of the material; I simply object to your specifying a certain way of doing the work and a certain way of constructing the mill. You have a right to ask for a certain quality in the rail, but no one will intimate that it helps the rail to wait ten seconds or thirty seconds before it is sawed. I say we must not legislate for or against any existing mill on account of a mere incidental condition that does not affect the quality of the rail at all.

As to the finishing temperature, it has been argued by Mr. Sauveur, by Mr. Martin, and I think it will be generally conceded, that the rail would be better if it were held at some previous pass. Shrinkage is going on while it is being held on the table of this Morrison-Kennedy process arrangement, and therefore they get a very small shrinkage after the last pass. Now then, can you ask any other mill to parallel that condition and give as small a shrinkage? I do not think it is just to ask it until you prove positively that a finishing pass with its 5% or 7% amount of work reduction does just as much good as a whole series of passes of 20%, 25% and 30%. Mr. McLeod has stated the thing very scientifically and has said that the evidence before them justified that procedure, but I do not think he will make a positive statement that it does do as much good; if not, then you must certainly not found all your specifications on the shrinkage obtained in that one process, and you must acknowledge that the Scranton mill and the Maryland Steel Company, both of which roll down in one heat from the ingot, have always turned out rails at a low temperature, and they do not get the

shrinkage asked by the Pennsylvania Railroad specifications; they can't do it and they don't know how to do it. There is one important difference, however, between an error by a railroad and an error by this Society. When the Pennsylvania Railroad makes a mistake, the inspector lets the mistake hang up in the air, while he takes his rails, for the power that wrote the specification can modify it on a moment's notice; but when we as an organization—I was going to say an international society—publicly proclaim a certain shrinkage clause, we cannot modify it without long debate and the lapse of many months. It seems to me that we must get positive information; we must rather be guided by standard practice; the clause before us is not standard practice, and is brought forward without complete proof of its value. Mr. Campbell.

MR. MCLEOD.—Mr. Campbell's sentiments, as he has just expressed them, agree with my own. We do not feel that this question is absolutely settled; but we feel certain that we are getting a better product than we did before; how much better the rails will have to tell for themselves, and we are perfectly willing that they should. Mr. McLeod.

MR. KENNEY.—I want to correct the impression that the Pennsylvania Railroad specifications were based on the Edgar Thomson practice. That is not the case. I went out to the Cambria mill personally, and in the presence of the Cambria officials rails were rolled and held longer and longer until we reached what we considered the proper finishing temperature. The shrinkage allowed was 5½ inches on an 85-pound rail, and those rails were put in track and gave better satisfaction than almost any we ever got from carbon steel. Mr. Kenney.

GEORGE E. THACKRAY.—Regarding the introduction of a shrinkage clause in the rail specifications, as at present understood, this is almost purely theoretical to-day, and although we have been working for some years to discover the relation between the structure of rails and their wear, we must confess that we have not yet been able to determine this definitely and positively. The relations between the shrinkage, the structure and the wearing quality are as yet unknown, and until we can make a specification that means something, based upon facts, this question should be held in abeyance. Mr. Thackray.

Mr. Webster

WM. R. WEBSTER.—There seems to be some misunderstanding about this matter. We have not, as members of the Rail Committee of the American Railway Engineering and Maintenance of Way Association, asked this Society or any other body to state the amount of shrinkage in the specifications, and we ourselves did not know just what amount to specify. We knew it should be different for a rail rolled direct from the ingot and from one rolled from a reheated bloom. But we do want data on this important matter in order that we may at some future time specify the proper shrinkage on rails.

Mr Sauveur

MR. SAUVEUR.—Referring to Dr. Dudley's inquiry whether such a slight reduction as 5 to 7½% would affect the structure deeply enough to produce a much better wearing surface, I of course cannot answer that question. I am inclined to think, however, that the effect of such a slight amount of work would be very superficial—that it would not extend far below the mere skin of the rail. If I remember rightly, Mr. Martin, of the Maryland Steel Company, did some experimental work in that direction, and found that the alteration of the structure was very superficial indeed.

Mr. Carhart.

P. E. CARHART.—At the Illinois Steel Company's mill the rolling is direct from the ingot, and I believe we are rolling at a considerably lower temperature than certain other leading mills. It is true, however, that the shrinkage which we are now getting is not as low as that obtained by the Kennedy-Morrison process. In my judgment, it would be a mistake for this Society to adopt a shrinkage clause at this time, for reasons already referred to by other speakers.

Mr. Webster.

MR. WEBSTER.—I move that this question be referred to the American Branch of Committee No. 1 for the purpose of gaining further information on the subject. [Motion carried.]

# THE RELATION BETWEEN THE BASIC OPEN-HEARTH PROCESS AND THE PHYSIÇAL PROPERTIES OF STEEL.

## Topical Discussion.

Charles B. Dudley.—During the past six or eight years, we have noticed in the tests of metal which have been made in connection with the Testing Department of the Pennsylvania Railroad Company, that the tensile strength of the steel, seemed to be diminishing. Specifications which used to furnish us steel showing 80 000 to 85 000 pounds tensile strength, now show steel of 70 000 to 75 000 pounds, and indeed even lower than this, and that, too, notwithstanding the chemical part of the specifications has been unchanged. Perhaps the most notable example of this is in the case of the specifications for car axles. These specifications require a steel of from 0.35 to 0.50% carbon, phosphorus not over 0.07%, manganese not over 0.60%, silicon about 0.05% and sulphur about 0.04%. Under these specifications some years ago, we expected to get, as said above, not less than 80 000 pounds tensile strength, with an elongation in 8 inches of from 16 to 18%. More recently, under the same specifications, we are finding the tensile strength to drop off quite perceptibly. It should be stated that during this period the basic open-hearth has replaced largely the acid open-hearth in the manufacture of steel for axles, and under this process, while the carbon and manganese remained much the same, the phosphorus, silicon and sulphur have dropped quite considerably. We think it would be expected that this diminution in these constituents, would to a certain extent at least affect the tensile strength, and as there are those present who have done a good deal of work on this very point, we shall look with very much interest to see whether, as the result of their studies, the change in chemical composition will account for the change in physical properties. We have ourselves planned quite an investigation on this point, embracing perhaps one hundred samples, but have not yet obtained any

Mr. Dudley.

Mr. Dudley. data. The fact, however, remains, and has a practical bearing in this, that all our experience now for eighteen to twenty years on this point, points quite clearly in the direction that for metal subject to alternate transverse stresses, as is the case in car axles, a metal of about 80 000 pounds tensile strength is extremely desirable, if not essential.

It should be stated for information that car axles are usually designed with a maximum fibre stress of about 20 000 pounds per square inch in tension, and a corresponding stress in compression. To successfully withstand these stresses without detail fracture, steel of the grade mentioned seems to be essential.

There is another phase of the question upon which it seems to us both discussion and investigation are desirable, namely, will the same chemical composition and the same heat treatment in two pieces of steel, one made in the acid open-hearth furnace and the other in the basic open-hearth furnace, give the same physical properties; in other words, chemical composition being laid aside and heat treatment being made the same, do the two processes give a steel with the same physical properties? We have some indications, but no definite proof that steel made by the basic open-hearth process, under these conditions, will not have the same physical properties as if it had been made in the acid furnace.

I may add for information that our present axle specifications are in reality over six years old, and that since they were put in force, some 380 000 axles of various sizes have been purchased and put under cars on those specifications. From some of the axles very little service has yet been obtained, as a very large number of them have been put in service during the past year. We have thus far had very few failures, but we are beginning to get indications that the older axles made in the basic furnace are not stiff enough for the service. In other words, they do not come up to the tensile strength mentioned as being desirable for car axles. It may also be interesting to know how it came about that this figure of about 80 000 pounds tensile strength was decided on as being satisfactory for car axles. Something over twenty-five years ago, most car axles were made of iron, and as steel was coming forward with prominence, it was deemed advisable to begin experiments with it. Accord-

ingly some axles were obtained of about 65 000 pounds tensile strength. Test axles of this kind showed quite remarkable behavior under the drop test, far outstripping iron axles of the same size in this respect, and it was thought that a very much better axle than an iron axle had been obtained. Mr. Dudley.

Accordingly the use of steel was quite rapidly extended, but in the course of two or three years it was found that these axles began to fail in service, due to detail fracture. The fracture was principally at the shoulder of the journal, and in some cases the journal was found in the car box at the end of the run, having dropped off in service. The matter became so serious that a consultation was called, with the hope of devising some means of overcoming this difficulty. There seemed to be two possible methods of meeting the problem, either to increase the size of the axles, or to stiffen the steel. To increase sizes meant redesigning the parts, which is always a serious matter with structures in service. Accordingly the other horn of the dilemma was taken, and a stiffer steel was demanded. The limit of 80 000 pounds was decided on, not because we had positive knowledge, but because it was hoped that such a strengthening of the metal would enable it to meet the strains successfully. The events proved that the increase made in the strength of the metal was a fortunate one. It is perhaps safe to say that it is very rare that a metal of 80 000 pounds tensile strength in car axles breaks in detail, with the present designs.

I am sorry not to have more definite information to communicate on these points, but as stated at the outset, I cannot at this time do more than simply state the problem.

MR. CAMPBELL.—Mr. Webster was one of the first in this country to make a systematic investigation of the influence of the metalloids upon steel, and I tried a little hand at it a short time afterward. The criticism that I have made of Mr. Webster's investigation applies to a less extent to the present question: any two test pieces, any two pieces of structural material, are made under different conditions; they are larger or smaller; they are made under different conditions of work and temperature; and it is hard to compare the chemical analysis and physical results of one piece with the chemical analysis and the physical results of another piece and say that the difference is due solely Mr. Campbell.

Mr. Campbell. to one factor. At the Pennsylvania Steel Company we take a test from every open-hearth heat, and also test pieces are taken under as nearly the same conditions as possible: in the casting, in the working, and in the finishing into the same sized test piece. They are heated always in the same furnace by the same men, worked in the same sized roll in the same number of passes, and finished as nearly as we can tell at exactly the same temperature and cooled in the same way and pulled in the same machine by the same men. Our experience of ten or fifteen years shows that the results obtained from those test pieces are conclusive, and are regular; in other words, when we sell steel to a certain plate-maker to roll plates, we tell him what the result of our test piece was for that heat. That does not mean that if he rolls a 1½-inch plate, he is going to get the same tensile strength that we found, but he can judge from that as a basis, and he will get about the same results as ours on a 3-8 or 7-16 or ½-inch plate rolled under ordinary conditions; from that he can judge what it would be on a thicker or thinner plate.

We have on record such a test piece from every heat that we have cast in our open-hearth plant for years, and we recognize that there is a certain determinative error in each one of these, this error amounting to possibly a few hundred, or possibly a thousand, pounds. In order to avoid these errors and also to make less work, I grouped the different kinds of heats that were alike, not grouping any that were unlike. Sometimes there were six in a group and sometimes forty, where they seemed to be all alike in chemical composition. Now, then, we have a large number of those averages—averages of tensile strength and chemical composition. The question was to determine the effect of the different elements, and this was done by the "Method of Least Squares." The result found by this method of calculation is not mere theory. No preconception has anything whatever to do with it. It is not a question of whether you believe the result or not. It is an algebraic answer. We did that for acid steel as distinct from basic steel, and our conclusion showed that carbon has less effect per unit on basic steel than it does on acid; that is the answer given by this investigation. I do not say that it is a fact, but simply that the figures so stated. This work was done about eight or nine years ago. From then until now

we have not shipped a single heat where the chemical composition has not been figured out by the formula to find out whether it agreed with the result from the pulling machine. Our men have become so thoroughly convinced that the basic formula fits the basic steel and the acid formula fits the acid steel, that if results do not agree within, say 2 000 pounds, they immediately look for an error and have the chemical determinations and the physical determinations repeated on the extra test piece. They are perfectly convinced that they will come out right, and they know very well that the acid formula will not fit the basic and the basic will not fit the acid steel. Mr. Campbell.

The carbon has less effect per unit on the basic steel than it does on the acid, from which it is clear that to get a basic steel with 70 000 pounds tensile strength, will demand a higher content of carbon than with an acid steel of the same tensile strength, even though the phosphorus were the same; because in both cases we allow the value for the phosphorus which the algebraic solution gives. We find that in both our basic and our acid equation the phosphorus confers strength, just about as much as carbon in round numbers; that is to say, that .01% of phosphorus amounts roughly to a thousand pounds in tensile strength; so that in reducing phosphorus from .07 to .02%, which would be five points, you would take 5 000 pounds off the tensile strength and you would have to add five points carbon to make up for it, but if it were basic steel, then the five points of carbon would not make up for it; you would have to put on about seven points. In low structural steel this difference in the value of carbon in acid and basic steels does not cut a very great figure, but in the 85 000 and 90 000 pounds it begins to be important in the larger amount called for of the metalloid.

THE PRESIDENT.—The practical question is this: If it be true, as our experience seems to indicate, that a car axle of about 65 000 pounds tensile strength steel is liable to break in detail under the conditions of service, and if it is true likewise, that under the specification with a limit of .35 to .50% carbon we are getting a good many axles that run as low, if made by the basic process, as 65 000 pounds, then we shall have to change our specifications; because we need a higher tensile strength steel The President.

The President. for axles to avoid failure in detail. That is the practical problem. Shall we increase the carbon to above .50%, or what shall we do? We must have, apparently, material of about the stiffness that is characteristic of an 80 000-pound steel in order to successfully resist the stresses in car axles. Now, how are we going to get it when the steel is made by the basic process?

Mr. Condron. MR. CONDRON.—With regard to phosphorus and its relation to the physical properties of steel, the question arises, is not there danger of endeavoring to get very low phosphorus? Specifications occasionally come into my hands, which call for phosphorus as low as .02%, and they frequently state that the analyses in basic open-hearth steel be considered below .01%. In the tests referred to by Mr. Campbell, the material has all been subjected to similar heat treatment, and as we are all aware, the material as delivered to the bridge builder is subjected to a great variation in heat treatment, which undoubtedly affects the physical properties quite as much as the chemical composition does.

Mr. Campbell. MR. CAMPBELL.—My opinion is that the low phosphorus, even when down to .005% or lower, does no harm at all. We cannot see that it makes any difference in the tensile strength, nor in rolling qualities, nor in the physical qualities, in uniformity, or in any other way. I do not think that other people have ever found anything of the kind, either. I would ask Dr. Dudley, in case he is going to print what he has just said, to question whether his determination of carbon some twenty-five years ago was correct when steel of .22% carbon gave only 65 000 pounds. I think he will find that twenty years ago the steel works were all wrong on their determinations of carbon in low steel. I know that our company, some twenty-five years ago, was wrong by thirteen points. I think at that time others were also wrong; and they were always determining carbons which ran from .08 to .14% as being .20 to .22%; more than that, they are all wrong now on very soft steels, for they continually give results as .07, .08, .10, .11 and .12% when the steel carries only .04, .05 and .06%, and they will even give you .08 and .09% when they are really .025 and .03%. It is perfectly immaterial, but still it is wrong.

The President. THE PRESIDENT.—In answer to Mr. Campbell's inquiry I would say that there are no positive records of the analyses of

the steel in question, but the works that made the steel, report that the carbon was from 0.25 to 0.28%, the phosphorus about 0.04%, the manganese from 0.50 to 0.55%, and the sulphur and silicon quite low. The President.

PAUL KREUZPOINTNER.—I am not able to speak in regard to the chemical properties of the steel used, nor to present such data of results as our large manufacturers can present, or Mr. Webster has been able to give us. At the same time, during those more than twenty years of testing on the Pennsylvania Railroad, which cover the period of transition of steel-making, I could not but have noticed some things. One of these things is, that not enough attention is paid to the elastic limit in determining the quality of the steel for structural purposes. The talk is always about the ultimate strength. Now then, I have noticed this: Not only in new material, but in scores and hundreds of axles—broken axles that have come back for examination and investigation—that the ultimate strength may be the same in the same material of different makers, but the elastic limit may vary very much; and in the end, as everyone may determine for himself, if he takes the time and trouble to do so, we find that when the first point of extension is passed, that one metal will go to destruction more rapidly than the other. Mr. Kreuzpointner.

This brings me to another point. It has been claimed that it does not seem to make any difference whether we use soft or hard steel in bridges: that soft steel will stand the vibrations just as well and that it is as good as harder steel. This is against all practical experience. The harder steel necessarily has harder crystals. Whether the matrix be harder or the crystals alone be harder does not matter. As soon as we put a stress on the steel the crystals begin to move: they slide upon each other; and if the particles are soft, then they will be crushed upon each other more easily than if the crystals are hard. If the matrix in which the crystals are imbedded is soft, then the crystals will slide more easily out of their position than if the matrix is hard. Now if the degree of elasticity of steel is dependent upon the degree of the hardness, or softness, of its matrix and crystals, or both, then, if the crystals are hard, and particularly if the matrix is also hard, then, on the applica-

Mr Kreuzpointner.

tion of stresses, the crystals will not be forced out of their position so readily as when the matrix is soft, nor will the crystals be crushed so easily upon each other when they begin to slide upon each other; and when the load, causing stress and dislocation of crystals is removed in time these harder crystals will more readily slide back into their original positions. And this ability of the crystals to slide back into their original positions before permanent set takes place, we call the elastic limit, or that point where the metal regains its original dimensions. Thus, then, if the crystals are hard enough and they are able to return, we have a higher elastic limit and the steel will have greater elasticity; and having greater elasticity, it will naturally be able to withstand more vibrations successively than a steel where the crystals are easily drawn out of their position, and are crushed. This may be the reason why harder steel will suffer less under vibrations and is less subject to breakages in detail than the softer steel.

I found that practically confirmed when the original specifications for axle steel were raised to 80 000 pounds, and when 75 000 pounds was the limit of acceptance. In every-day practice it was found that as soon as the steel was accepted at less than 75 000 pounds, then detail fracture would take place.

To avoid detail fracture, it would seem well, therefore, to rely not merely on the chemical composition and the ultimate strength, but also to pay a little more attention to the elastic limit.

Since the methods of manufacture are so very much more uniform nowadays than fifteen or twenty years ago, I am of the opinion that if we used fewer test pieces, but subjected these to a more rigid and more scientific investigation, we would perhaps get better results, and solve some of the problems which are now puzzling the engineer and the mill man very much.

Mr. Webster.

WM. R. WEBSTER (by letter).—The subject presented for discussion by Dr. Dudley is of such importance that I have prepared the following table, which gives the estimated ultimate tensile strength by the methods of Mr. Cunningham, Mr. Campbell and myself. The values are given for both acid and basic open-hearth steel, for each point of phosphorus, up to .08 inclusive, in connection with each five points of carbon from .06

# COMPARISON OF ESTIMATED ULTIMATE STRENGTHS OF OPEN-HEAR

| Carbon and Manganese. | | Phosphorus .01 | | | | Phosphorus .02 | | | | Phosphorus .03 | | | | Phosphorus .04 | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Cunningham. | Campbell. | | Webster. | Cunningham. | Campbell. | | Webster. | Cunningham. | Campbell. | | Webster. | Cunningham. | Campbell. | | |
| C. | Mn. | | Acid. | Basic. | | | Acid. | Basic. | | | Acid. | Basic. | | | Acid. | Basic. | |
| .06 | .20 | 47 000 | 46 750 | 45 880 | 46 300 | 48 000 | 47 640 | 46 930 | 47 100 | 49 000 | 48 530 | 47 980 | 47 900 | 50 000 | 49 420 | 49 030 | |
| | .25 | " | " | 46 305 | 47 400 | " | " | 47 355 | 48 200 | " | " | 48 405 | 49 000 | " | " | 49 455 | |
| | .30 | " | " | 46 730 | 48 400 | " | " | 47 780 | 49 200 | " | " | 48 830 | 50 000 | " | " | 49 880 | |
| | .35 | " | " | 47 155 | 49 300 | " | " | 48 205 | 50 100 | " | " | 49 255 | 50 900 | " | " | 50 305 | |
| | .40 | " | " | 47 580 | 50 100 | " | " | 48 630 | 50 900 | " | " | 49 680 | 51 700 | " | " | 50 730 | |
| | .45 | " | " | 48 005 | 50 800 | " | " | 49 055 | 51 600 | " | " | 50 105 | 52 400 | " | " | 51 155 | |
| | .50 | " | " | 48 430 | 51 400 | " | " | 49 480 | 52 200 | " | " | 50 530 | 53 000 | " | " | 51 580 | |
| | .55 | " | " | 48 855 | 51 900 | " | " | 49 905 | 52 700 | " | " | 50 955 | 53 500 | " | " | 52 005 | |
| | .60 | " | | 49 280 | 52 400 | " | " | 50 330 | 53 200 | " | " | 51 380 | 54 000 | " | " | 52 430 | |
| .10 | .20 | 51 000 | 51 590 | 49 680 | 49 700 | 52 000 | 52 480 | 50 730 | 50 700 | 53 000 | 53 370 | 51 780 | 51 700 | 54 000 | 54 260 | 52 830 | |
| | .25 | " | " | 50 105 | 50 800 | " | " | 51 155 | 51 800 | " | " | 52 205 | 52 800 | " | " | 53 255 | |
| | .30 | " | " | 50 530 | 51 800 | " | " | 51 580 | 52 800 | " | " | 52 630 | 53 800 | " | " | 53 680 | |
| | .35 | " | " | 50 955 | 52 700 | " | " | 52 005 | 53 700 | " | " | 53 055 | 54 700 | " | " | 54 105 | |
| | .40 | " | " | 51 380 | 53 500 | " | " | 52 430 | 54 500 | " | " | 53 480 | 55 500 | " | " | 54 530 | |
| | .45 | " | " | 51 805 | 54 200 | " | " | 52 855 | 55 200 | " | " | 53 905 | 56 200 | " | " | 54 955 | |
| | .50 | " | " | 52 230 | 54 800 | " | " | 53 280 | 55 800 | " | " | 54 330 | 56 800 | " | " | 55 380 | |
| | .55 | " | " | 52 655 | 55 300 | " | " | 53 705 | 56 300 | " | " | 54 755 | 57 300 | " | " | 55 805 | |
| | .60 | " | " | 53 080 | 55 800 | " | " | 54 130 | 56 800 | " | " | 55 180 | 57 800 | " | " | 56 230 | |
| .15 | .20 | 56 000 | 57 640 | 54 430 | 54 200 | 57 000 | 58 530 | 55 480 | 55 700 | 58 000 | 59 420 | 56 530 | 57 200 | 59 000 | 60 310 | 57 580 | |
| | .25 | " | " | 54 855 | 55 300 | " | " | 55 905 | 56 800 | " | " | 56 955 | 58 300 | " | " | 58 005 | |
| | .30 | " | " | 55 280 | 56 300 | " | " | 56 330 | 57 800 | " | " | 57 380 | 59 300 | " | " | 58 430 | |
| | .35 | " | " | 55 705 | 57 200 | " | " | 56 755 | 58 700 | " | " | 57 805 | 60 200 | " | " | 58 855 | |
| | .40 | " | " | 56 130 | 58 000 | " | " | 57 180 | 59 500 | " | " | 58 230 | 61 000 | " | " | 59 280 | |
| | .45 | " | " | 56 555 | 58 700 | " | " | 57 605 | 60 200 | " | " | 58 655 | 61 700 | " | " | 59 705 | |
| | .50 | " | " | 56 980 | 59 300 | " | " | 58 030 | 60 800 | " | " | 59 080 | 62 300 | " | " | 60 130 | |
| | .55 | " | " | 57 405 | 59 800 | " | " | 58 455 | 61 300 | " | " | 59 505 | 62 800 | " | " | 60 555 | |
| | .60 | " | " | 57 830 | 60 300 | " | " | 58 880 | 61 800 | " | " | 59 930 | 63 300 | " | " | 60 980 | |
| .20 | .20 | 61 000 | 63 690 | 59 180 | 58 200 | 62 000 | 64 580 | 60 230 | 59 700 | 63 000 | 65 470 | 61 280 | 61 200 | 64 000 | 66 360 | 62 330 | |
| | .25 | " | " | 59 605 | 59 300 | " | " | 60 655 | 60 800 | " | " | 61 705 | 62 300 | " | " | 62 755 | |
| | .30 | " | " | 60 030 | 60 300 | " | " | 61 080 | 61 800 | " | " | 62 130 | 63 300 | " | " | 63 180 | |
| | .35 | " | " | 60 455 | 61 200 | " | " | 61 505 | 62 700 | " | " | 62 555 | 64 200 | " | " | 63 605 | |
| | .40 | " | " | 60 880 | 62 000 | " | " | 61 930 | 63 500 | " | " | 62 980 | 65 000 | " | " | 64 030 | |
| | .45 | " | " | 61 305 | 62 700 | " | " | 62 355 | 64 200 | " | " | 63 405 | 65 700 | " | " | 64 455 | |
| | .50 | " | " | 61 730 | 63 300 | " | " | 62 780 | 64 800 | " | " | 63 830 | 66 300 | " | " | 64 880 | |
| | .55 | " | " | 62 155 | 63 800 | " | " | 63 205 | 65 300 | " | " | 64 255 | 66 800 | " | " | 65 305 | |
| | .60 | " | " | 62 580 | 64 300 | " | " | 63 630 | 65 800 | " | " | 64 680 | 67 300 | " | " | 65 730 | |
| .25 | .20 | 66 000 | 69 740 | 63 930 | 62 200 | 67 000 | 70 630 | 64 980 | 63 700 | 68 000 | 71 520 | 66 030 | 65 200 | 69 000 | 72 410 | 67 080 | |
| | .25 | " | " | 64 355 | 63 300 | " | " | 65 405 | 64 800 | " | " | 66 455 | 66 300 | " | " | 67 505 | |
| | .30 | " | " | 64 780 | 64 300 | " | " | 65 830 | 65 800 | " | " | 66 880 | 67 300 | " | " | 67 930 | |
| | .35 | " | " | 65 205 | 65 200 | " | " | 66 255 | 66 700 | " | " | 67 305 | 68 200 | " | " | 68 355 | |
| | .40 | " | " | 65 630 | 66 000 | " | " | 66 680 | 67 500 | " | " | 67 730 | 69 000 | " | " | 68 780 | |
| | .45 | " | " | 66 055 | 66 700 | " | " | 67 105 | 68 200 | " | " | 68 155 | 69 700 | " | " | 69 205 | |
| | .50 | " | " | 66 480 | 67 300 | " | " | 67 530 | 68 800 | " | " | 68 580 | 70 300 | " | " | 69 630 | |
| | .55 | " | " | 66 905 | 67 800 | " | " | 67 955 | 69 300 | " | " | 69 005 | 70 800 | " | " | 70 055 | |
| | .60 | " | " | 67 330 | 68 300 | " | " | 68 380 | 69 800 | " | " | 69 430 | 71 300 | " | " | 70 480 | |
| .30 | .20 | 71 000 | 75 790 | 68 680 | 66 200 | 72 000 | 76 680 | 69 730 | 67 700 | 73 000 | 77 570 | 70 780 | 69 200 | 74 000 | 78 460 | 71 830 | |
| | .25 | " | " | 69 105 | 67 300 | " | " | 70 155 | 68 800 | " | " | 71 205 | 70 300 | " | " | 72 255 | |
| | .30 | " | " | 69 530 | 68 300 | " | " | 70 580 | 69 800 | " | " | 71 630 | 71 300 | " | " | 72 680 | |
| | .35 | " | " | 69 955 | 69 200 | " | " | 71 005 | 70 700 | " | " | 72 055 | 72 200 | " | " | 73 105 | |
| | .40 | " | " | 70 380 | 70 000 | " | " | 71 430 | 71 500 | " | " | 72 480 | 73 000 | " | " | 73 530 | |
| | .45 | " | " | 70 805 | 70 700 | " | " | 71 855 | 72 200 | " | " | 72 905 | 73 700 | " | " | 73 955 | |
| | .50 | " | " | 71 230 | 71 300 | " | " | 72 280 | 72 800 | " | " | 73 330 | 74 300 | " | " | 74 380 | |
| | .55 | " | " | 71 655 | 71 800 | " | " | 72 705 | 73 300 | " | " | 73 755 | 74 800 | " | " | 74 805 | |
| | .60 | " | " | 72 080 | 72 300 | " | " | 73 130 | 73 800 | " | " | 74 180 | 75 300 | " | " | 75 230 | |

Note.—Cunningham's and Webster's values are for either Acid or Basic Open-Hearth steel.

# TH STEEL BY CUNNINGHAM'S, CAMPBELL'S AND WEBSTER'S METHODS.

| Phosphorus .05 | | | | Phosphorus .06. | | | | Phosphorus .07 | | | | Phosphorus .08 | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Webster. | Cunningham. | Campbell. Acid. | Campbell. Basic. | Webster. | Cunningham. | Campbell. Acid. | Campbell. Basic. | Webster. | Cunningham. | Campbell. Acid. | Campbell. Basic. | Webster. | Cunningham. | Campbell. Acid. | Campbell. Basic. | Webster. |
| 48 700 | 51 000 | 50 310 | 50 080 | 49 500 | 52 000 | 51 200 | 51 130 | 50 300 | 53 000 | 52 090 | 52 180 | 51 100 | 54 000 | 52 980 | 53 230 | 51 900 |
| 49 800 | " | " | 50 505 | 50 600 | " | " | 51 555 | 51 400 | " | " | 52 605 | 52 200 | " | " | 53 655 | 53 000 |
| 50 800 | " | " | 50 930 | 51 600 | " | " | 51 980 | 52 400 | " | " | 53 030 | 53 200 | " | " | 54 080 | 54 000 |
| 51 700 | " | " | 51 355 | 52 500 | " | " | 52 405 | 53 300 | " | " | 53 455 | 54 100 | " | " | 54 505 | 54 900 |
| 52 500 | " | " | 51 780 | 53 300 | " | " | 52 830 | 54 100 | " | " | 53 880 | 54 900 | " | " | 54 930 | 55 700 |
| 53 200 | " | " | 52 205 | 54 000 | " | " | 53 255 | 54 800 | " | " | 54 305 | 55 600 | " | " | 55 355 | 56 400 |
| 53 800 | " | " | 52 630 | 54 600 | " | " | 53 680 | 55 400 | " | " | 54 730 | 56 200 | " | " | 55 780 | 57 000 |
| 54 300 | " | " | 53 055 | 55 100 | " | " | 54 105 | 55 900 | " | " | 55 155 | 56 700 | " | " | 56 205 | 57 500 |
| 54 800 | " | " | 53 480 | 55 600 | " | " | 54 530 | 56 400 | " | " | 55 580 | 57 200 | " | " | 56 630 | 58 000 |
| 52 700 | 55 000 | 55 150 | 53 880 | 53 700 | 56 000 | 56 040 | 54 930 | 54 700 | 57 000 | 56 930 | 55 980 | 55 700 | 58 000 | 57 820 | 57 030 | 56 700 |
| 53 800 | " | " | 54 305 | 54 800 | " | " | 55 355 | 55 800 | " | " | 56 405 | 56 800 | " | " | 57 455 | 57 800 |
| 54 800 | " | " | 54 730 | 55 800 | " | " | 55 780 | 56 800 | " | " | 56 830 | 57 800 | " | " | 57 880 | 58 800 |
| 55 700 | " | " | 55 155 | 56 700 | " | " | 56 205 | 57 700 | " | " | 57 255 | 58 700 | " | " | 58 305 | 59 700 |
| 56 500 | " | " | 55 580 | 57 500 | " | " | 56 630 | 58 500 | " | " | 57 680 | 59 500 | " | " | 58 730 | 60 500 |
| 57 200 | " | " | 56 005 | 58 200 | " | " | 57 055 | 59 200 | " | " | 58 105 | 60 200 | " | " | 59 155 | 61 200 |
| 57 800 | " | " | 56 430 | 58 800 | " | " | 57 480 | 59 800 | " | " | 58 530 | 60 800 | " | " | 59 580 | 61 800 |
| 58 300 | " | " | 56 855 | 59 300 | " | " | 57 905 | 60 300 | " | " | 58 955 | 61 300 | " | " | 60 005 | 62 300 |
| 58 800 | " | " | 57 280 | 59 800 | " | " | 58 330 | 60 800 | " | " | 59 380 | 61 800 | " | " | 60 430 | 62 800 |
| 58 700 | 60 000 | 61 200 | 58 630 | 60 200 | 61 000 | 61 090 | 59 680 | 61 700 | 62 000 | 61 980 | 60 730 | 63 200 | 63 000 | 62 870 | 61 780 | 64 700 |
| 59 800 | " | " | 59 055 | 61 300 | " | " | 60 105 | 62 800 | " | " | 61 155 | 64 300 | " | " | 62 205 | 65 800 |
| 60 800 | " | " | 59 480 | 62 300 | " | " | 60 530 | 63 800 | " | " | 61 580 | 65 300 | " | " | 62 630 | 66 800 |
| 61 700 | " | " | 59 905 | 63 200 | " | " | 60 955 | 64 700 | " | " | 62 005 | 66 200 | " | " | 63 055 | 67 700 |
| 62 500 | " | " | 60 330 | 64 000 | " | " | 61 380 | 65 500 | " | " | 62 430 | 67 000 | " | " | 63 480 | 68 500 |
| 63 200 | " | " | 60 755 | 64 700 | " | " | 61 805 | 66 200 | " | " | 62 855 | 67 700 | " | " | 63 905 | 69 200 |
| 63 800 | " | " | 61 180 | 65 300 | " | " | 62 230 | 66 800 | " | " | 63 280 | 68 300 | " | " | 64 330 | 69 800 |
| 64 300 | " | " | 61 605 | 65 800 | " | " | 62 655 | 67 300 | " | " | 63 705 | 68 800 | " | " | 64 755 | 70 300 |
| 64 800 | " | " | 62 030 | 66 300 | " | " | 63 080 | 67 800 | " | " | 64 130 | 69 300 | " | " | 65 180 | 70 800 |
| 62 700 | 65 000 | 67 250 | 63 380 | 64 200 | 66 000 | 68 140 | 64 430 | 65 700 | 67 000 | 69 030 | 65 480 | 67 200 | 68 000 | 69 920 | 66 530 | 68 700 |
| 63 800 | " | " | 63 805 | 65 300 | " | " | 64 855 | 66 800 | " | " | 65 905 | 68 300 | " | " | 66 955 | 69 800 |
| 64 800 | " | " | 64 230 | 66 300 | " | " | 65 280 | 67 800 | " | " | 66 330 | 69 300 | " | " | 67 380 | 70 800 |
| 65 700 | " | " | 64 655 | 67 200 | " | " | 65 705 | 68 700 | " | " | 66 755 | 70 200 | " | " | 67 805 | 71 700 |
| 66 500 | " | " | 65 080 | 68 000 | " | " | 66 130 | 69 500 | " | " | 67 180 | 71 000 | " | " | 68 230 | 72 500 |
| 67 200 | " | " | 65 505 | 68 700 | " | " | 66 555 | 70 200 | " | " | 67 605 | 71 700 | " | " | 68 655 | 73 200 |
| 67 800 | " | " | 65 930 | 69 300 | " | " | 66 980 | 70 800 | " | " | 68 030 | 72 300 | " | " | 69 080 | 73 800 |
| 68 300 | " | " | 66 355 | 69 800 | " | " | 67 405 | 71 300 | " | " | 68 455 | 72 800 | " | " | 69 505 | 74 300 |
| 68 800 | " | " | 66 780 | 70 300 | " | " | 67 830 | 71 800 | " | " | 68 880 | 73 300 | " | " | 69 930 | 74 800 |
| 66 700 | 70 000 | 73 300 | 68 130 | 68 200 | 71 000 | 74 190 | 69 180 | 69 700 | 72 000 | 75 080 | 70 230 | 71 200 | 73 000 | 75 970 | 71 280 | 72 700 |
| 67 800 | " | " | 68 555 | 69 300 | " | " | 69 605 | 70 800 | " | " | 70 655 | 72 300 | " | " | 71 705 | 73 800 |
| 68 800 | " | " | 68 980 | 70 300 | " | " | 70 030 | 71 800 | " | " | 71 080 | 73 000 | " | " | 72 130 | 74 800 |
| 69 700 | " | " | 69 405 | 71 200 | " | " | 70 455 | 72 700 | " | " | 71 505 | 74 200 | " | " | 72 555 | 75 700 |
| 70 500 | " | " | 69 830 | 72 000 | " | " | 70 880 | 73 500 | " | " | 71 930 | 75 000 | " | " | 72 980 | 76 500 |
| 71 200 | " | " | 70 255 | 72 700 | " | " | 71 305 | 74 200 | " | " | 72 355 | 75 700 | " | " | 73 405 | 77 200 |
| 71 800 | " | " | 70 680 | 73 300 | " | " | 71 730 | 74 800 | " | " | 72 780 | 76 300 | " | " | 73 830 | 77 800 |
| 72 300 | " | " | 71 105 | 73 800 | " | " | 72 155 | 75 300 | " | " | 73 205 | 76 800 | " | " | 74 255 | 78 300 |
| 72 800 | " | " | 71 530 | 74 300 | " | " | 72 580 | 75 800 | " | " | 73 630 | 77 300 | " | " | 74 680 | 78 800 |
| 70 700 | 75 000 | 79 350 | 72 880 | 72 200 | 76 000 | 80 240 | 73 930 | 73 700 | 77 000 | 81 130 | 74 980 | 75 200 | 78 000 | 82 020 | 76 030 | 76 700 |
| 71 800 | " | " | 73 305 | 73 300 | " | " | 74 355 | 74 800 | " | " | 75 405 | 76 300 | " | " | 76 455 | 77 800 |
| 72 800 | " | " | 73 730 | 74 300 | " | " | 74 780 | 75 800 | " | " | 75 830 | 77 300 | " | " | 76 880 | 78 800 |
| 73 700 | " | " | 74 155 | 75 200 | " | " | 75 205 | 76 700 | " | " | 76 255 | 78 200 | " | " | 77 305 | 79 700 |
| 74 500 | " | " | 74 580 | 76 000 | " | " | 75 630 | 77 500 | " | " | 76 680 | 79 000 | " | " | 77 730 | 80 500 |
| 75 200 | " | " | 75 005 | 76 700 | " | " | 76 055 | 78 200 | " | " | 77 105 | 79 700 | " | " | 78 155 | 81 200 |
| 75 800 | " | " | 75 430 | 77 300 | " | " | 76 480 | 78 800 | " | " | 77 530 | 80 300 | " | " | 78 580 | 81 800 |
| 76 300 | " | " | 75 855 | 77 800 | " | " | 76 905 | 79 300 | " | " | 77 955 | 80 800 | " | " | 79 005 | 82 300 |
| 76 800 | " | " | 76 280 | 78 300 | " | " | 77 330 | 79 800 | " | " | 78 380 | 81 300 | " | " | 79 430 | 82 800 |

**Value of Carbon:**

| | |
|---|---|
| Cunningham | .01 C = 1000 lbs. |
| Campbell, Acid | .01 C = 1210 lbs. |
| Campbell, Basic | .01 C = 950 lbs. |
| Webster | .01 C = 800 lbs. |

# COMPARISON OF ESTIMATED ULTIMATE STRENGTHS OF OPEN-HEART

| Carbon and Manganese. | | Phosphorus .01 | | | | Phosphorus .02 | | | | Phosphorus .03 | | | | Phosphorus .04 | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Cunningham. | Campbell. | | Webster. | Cunningham. | Campbell. | | Webster. | Cunningham. | Campbell. | | Webster. | Cunningham. | Campbell. | | W s |
| C. | Mn. | | Acid. | Basic. | | | Acid. | Basic. | | | Acid. | Basic. | | | Acid. | Basic. | |
| .35 | .20 | 76 000 | 81 840 | 73 430 | 70 200 | 77 000 | 82 730 | 74 480 | 71 700 | 78 000 | 83 620 | 75 530 | 73 200 | 79 000 | 84 510 | 76 580 | 7 |
| | .25 | " | " | 73 855 | 71 300 | " | " | 74 905 | 72 800 | " | " | 75 955 | 74 300 | " | " | 77 005 | 7 |
| | .30 | " | " | 74 280 | 72 300 | " | " | 75 330 | 73 800 | " | " | 76 380 | 75 300 | " | " | 77 430 | 7 |
| | .35 | " | " | 74 705 | 73 200 | " | " | 75 755 | 74 700 | " | " | 76 805 | 76 200 | " | " | 77 855 | 7 |
| | .40 | " | " | 75 130 | 74 000 | " | " | 76 180 | 75 500 | " | " | 77 230 | 77 000 | " | " | 78 280 | 7 |
| | .45 | " | " | 75 555 | 74 700 | " | " | 76 605 | 76 200 | " | " | 77 655 | 77 700 | " | " | 78 705 | 7 |
| | .50 | " | " | 75 980 | 75 300 | " | " | 77 030 | 76 800 | " | " | 78 080 | 78 300 | " | " | 79 130 | 7 |
| | .55 | " | " | 76 405 | 75 800 | " | " | 77 455 | 77 300 | " | " | 78 505 | 78 800 | " | " | 79 555 | 8 |
| | .60 | " | " | 76 830 | 76 300 | " | " | 77 880 | 77 800 | " | " | 78 930 | 79 300 | " | " | 79 980 | 8 |
| .40 | .20 | 81 000 | 87 890 | 78 180 | 74 200 | 82 000 | 88 780 | 79 230 | 75 700 | 83 000 | 89 670 | 80 280 | 77 200 | 84 000 | 90 560 | 81 330 | 7 |
| | .25 | " | " | 78 605 | 75 300 | " | " | 79 655 | 76 800 | " | " | 80 705 | 78 300 | " | " | 81 755 | 7 |
| | .30 | " | " | 79 030 | 76 300 | " | " | 80 080 | 77 800 | " | " | 81 130 | 79 300 | " | " | 82 180 | 8 |
| | .35 | " | " | 79 455 | 77 200 | " | " | 80 505 | 78 700 | " | " | 81 555 | 80 200 | " | " | 82 605 | 8 |
| | .40 | " | " | 79 880 | 78 000 | " | " | 80 930 | 79 500 | " | " | 81 980 | 81 000 | " | " | 83 030 | 8 |
| | .45 | " | " | 80 305 | 78 700 | " | " | 81 355 | 80 200 | " | " | 82 405 | 81 700 | " | " | 83 455 | 8 |
| | .50 | " | " | 80 730 | 79 300 | " | " | 81 780 | 80 800 | " | " | 82 830 | 82 300 | " | " | 83 880 | 8 |
| | .55 | " | " | 81 155 | 79 800 | " | " | 82 205 | 81 300 | " | " | 83 255 | 82 800 | " | " | 84 305 | 8 |
| | .60 | " | " | 81 580 | 80 300 | " | " | 82 630 | 81 800 | " | " | 83 680 | 83 300 | " | " | 84 730 | 8 |
| .45 | .20 | 86 000 | 93 940 | 82 930 | 78 200 | 87 000 | 94 830 | 83 980 | 79 700 | 88 000 | 95 720 | 85 030 | 81 200 | 89 000 | 96 610 | 86 080 | 8 |
| | .25 | " | " | 83 355 | 79 300 | " | " | 84 405 | 80 800 | " | " | 85 455 | 82 300 | " | " | 86 505 | 8 |
| | .30 | " | " | 83 780 | 80 300 | " | " | 84 830 | 81 800 | " | " | 85 880 | 83 300 | " | " | 86 930 | 8 |
| | .35 | " | " | 84 205 | 81 200 | " | " | 85 255 | 82 700 | " | " | 86 305 | 84 200 | " | " | 87 355 | 8 |
| | .40 | " | " | 84 630 | 82 000 | " | " | 85 680 | 83 500 | " | " | 86 730 | 85 000 | " | " | 87 780 | 8 |
| | .45 | " | " | 85 055 | 82 700 | " | " | 86 105 | 84 200 | " | " | 87 155 | 85 700 | " | " | 88 205 | 8 |
| | .50 | " | " | 85 480 | 83 300 | " | " | 86 530 | 84 800 | " | " | 87 580 | 86 300 | " | " | 88 630 | 8 |
| | .55 | " | " | 85 905 | 83 800 | " | " | 86 955 | 85 300 | " | " | 88 005 | 86 800 | " | " | 89 055 | 8 |
| | .60 | " | " | 86 330 | 84 300 | " | " | 87 380 | 85 800 | " | " | 88 430 | 87 300 | " | " | 89 480 | 8 |
| .50 | .20 | 91 000 | 99 990 | 87 680 | 82 200 | 92 000 | 100 880 | 88 730 | 83 700 | 93 000 | 101 770 | 89 780 | 85 200 | 94 000 | 102 660 | 90 830 | 8 |
| | .25 | " | " | 88 105 | 83 300 | " | " | 89 155 | 84 800 | " | " | 90 205 | 86 300 | " | " | 91 255 | 8 |
| | .30 | " | " | 88 530 | 84 300 | " | " | 89 580 | 85 800 | " | " | 90 630 | 87 300 | " | " | 91 680 | 8 |
| | .35 | " | " | 88 955 | 85 200 | " | " | 90 005 | 86 700 | " | " | 91 055 | 88 200 | " | " | 92 105 | 8 |
| | .40 | " | " | 89 380 | 86 000 | " | " | 90 430 | 87 500 | " | " | 91 480 | 89 000 | " | " | 92 530 | 9 |
| | .45 | " | " | 89 805 | 86 700 | " | " | 90 855 | 88 200 | " | " | 91 905 | 89 700 | " | " | 92 955 | 9 |
| | .50 | " | " | 90 230 | 87 300 | " | " | 91 280 | 88 800 | " | " | 92 330 | 90 300 | " | " | 93 380 | 9 |
| | .55 | " | " | 90 655 | 87 800 | " | " | 91 705 | 89 300 | " | " | 92 755 | 90 800 | " | " | 93 805 | 9 |
| | .60 | " | " | 91 080 | 88 300 | " | " | 92 130 | 89 800 | " | " | 93 180 | 91 300 | " | " | 94 230 | 9 |
| .55 | .20 | 96 000 | 106 040 | 92 430 | 86 200 | 97 000 | 106 930 | 93 480 | 87 700 | 98 000 | 107 820 | 94 530 | 89 200 | 99 000 | 108 710 | 95 580 | 9 |
| | .25 | " | " | 92 855 | 87 300 | " | " | 93 905 | 88 800 | " | " | 94 955 | 90 300 | " | " | 96 005 | 9 |
| | .30 | " | " | 93 280 | 88 300 | " | " | 94 330 | 89 800 | " | " | 95 380 | 91 300 | " | " | 96 430 | 9 |
| | .35 | " | " | 93 705 | 89 200 | " | " | 94 755 | 90 700 | " | " | 95 805 | 92 200 | " | " | 96 855 | 9 |
| | .40 | " | " | 94 130 | 90 000 | " | " | 95 180 | 91 500 | " | " | 96 230 | 93 000 | " | " | 97 280 | 9 |
| | .45 | " | " | 94 555 | 90 700 | " | " | 95 605 | 92 200 | " | " | 96 655 | 93 700 | " | " | 97 705 | 9 |
| | .50 | " | " | 94 980 | 91 300 | " | " | 96 030 | 92 800 | " | " | 97 080 | 94 300 | " | " | 98 130 | 9 |
| | .55 | " | " | 95 405 | 91 800 | " | " | 96 455 | 93 300 | " | " | 97 505 | 94 800 | " | " | 98 555 | 9 |
| | .60 | " | " | 95 830 | 92 300 | " | " | 96 880 | 93 800 | " | " | 97 930 | 95 300 | " | " | 98 980 | 9 |
| .60 | .20 | 101 000 | 112 090 | 97 180 | 90 200 | 102 000 | 112 980 | 98 230 | 91 700 | 103 000 | 113 870 | 99 280 | 93 200 | 104 000 | 114 760 | 100 330 | 9 |
| | .25 | " | " | 97 605 | 91 300 | " | " | 98 655 | 92 800 | " | " | 99 705 | 94 300 | " | " | 100 755 | 9 |
| | .30 | " | " | 98 030 | 92 300 | " | " | 99 080 | 93 800 | " | " | 100 130 | 95 300 | " | " | 101 180 | 9 |
| | .35 | " | " | 98 455 | 93 200 | " | " | 99 505 | 94 700 | " | " | 100 555 | 96 200 | " | " | 101 605 | 9 |
| | .40 | " | " | 98 880 | 94 000 | " | " | 99 930 | 95 500 | " | " | 100 980 | 97 000 | " | " | 102 030 | 9 |
| | .45 | " | " | 99 305 | 94 700 | " | " | 100 355 | 96 200 | " | " | 101 405 | 97 700 | " | " | 102 455 | 9 |
| | .50 | " | " | 99 730 | 95 300 | " | " | 100 780 | 96 800 | " | " | 101 830 | 98 300 | " | " | 102 880 | 9 |
| | .55 | " | " | 100 155 | 95 800 | " | " | 101 205 | 97 300 | " | " | 102 255 | 98 800 | " | " | 103 305 | 10 |
| | .60 | " | " | 100 580 | 96 300 | " | " | 101 630 | 97 800 | " | " | 102 680 | 99 300 | " | " | 103 730 | 10 |

Note.—Cunningham's and Webster's values are for either Acid or Basic Open-Hearth steel.

# STEEL BY CUNNINGHAM'S, CAMPBELL'S AND WEBSTER'S METHODS.

| Phosphorus .05 | | | | Phosphorus .06 | | | | Phosphorus .07 | | | | Phosphorus .08 | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Cunningham. | Campbell. Acid. | Campbell. Basic. | Webster. | Cunningham. | Campbell. Acid. | Campbell. Basic. | Webster. | Cunningham. | Campbell. Acid. | Campbell. Basic. | Webster. | Cunningham. | Campbell. Acid. | Campbell. Basic. | Webster. |
| 80 000 | 85 400 | 77 630 | 76 200 | 81 000 | 86 290 | 78 680 | 77 700 | 82 000 | 87 180 | 79 730 | 79 200 | 83 000 | 88 070 | 80 780 | 80 700 |
| " | " | 78 055 | 77 300 | " | " | 79 105 | 78 800 | " | " | 80 155 | 80 300 | " | " | 81 205 | 81 800 |
| " | " | 78 480 | 78 300 | " | " | 79 530 | 79 800 | " | " | 80 580 | 81 300 | " | " | 81 630 | 82 800 |
| " | " | 78 905 | 79 200 | " | " | 79 955 | 80 700 | " | " | 81 005 | 82 200 | " | " | 82 055 | 83 700 |
| " | " | 79 330 | 80 000 | " | " | 80 380 | 81 500 | " | " | 81 430 | 83 000 | " | " | 82 480 | 84 500 |
| " | " | 79 755 | 80 700 | " | " | 80 805 | 82 200 | " | " | 81 855 | 83 700 | " | " | 82 905 | 85 200 |
| " | " | 80 180 | 81 300 | " | " | 81 230 | 82 800 | " | " | 82 280 | 84 300 | " | " | 83 330 | 85 800 |
| " | " | 80 605 | 81 800 | " | " | 81 655 | 83 300 | " | " | 82 705 | 84 800 | " | " | 83 755 | 86 300 |
| " | " | 81 030 | 82 300 | " | " | 82 080 | 83 800 | " | " | 83 130 | 85 300 | " | " | 84 180 | 86 800 |
| 85 000 | 91 450 | 82 380 | 80 200 | 86 000 | 92 340 | 83 430 | 81 700 | 87 000 | 93 230 | 84 480 | 83 200 | 88 000 | 94 120 | 85 530 | 84 700 |
| " | " | 82 805 | 81 300 | " | " | 83 855 | 82 800 | " | " | 84 905 | 84 300 | " | " | 85 955 | 85 800 |
| " | " | 83 230 | 82 300 | " | " | 84 280 | 83 800 | " | " | 85 330 | 85 300 | " | " | 86 380 | 86 800 |
| " | " | 83 655 | 83 200 | " | " | 84 705 | 84 700 | " | " | 85 755 | 86 200 | " | " | 86 805 | 87 700 |
| " | " | 84 080 | 84 000 | " | " | 85 130 | 85 500 | " | " | 86 180 | 87 000 | " | " | 87 230 | 88 500 |
| " | " | 84 505 | 84 700 | " | " | 85 555 | 86 200 | " | " | 86 605 | 87 700 | " | " | 87 655 | 89 200 |
| " | " | 84 930 | 85 300 | " | " | 85 980 | 86 800 | " | " | 87 030 | 88 300 | " | " | 88 080 | 89 800 |
| " | " | 85 355 | 85 800 | " | " | 86 405 | 87 300 | " | " | 87 455 | 88 800 | " | " | 88 505 | 90 300 |
| " | " | 85 780 | 86 300 | " | " | 86 830 | 87 800 | " | " | 87 880 | 89 300 | " | " | 88 930 | 90 800 |
| 90 000 | 97 500 | 87 130 | 84 200 | 91 000 | 98 390 | 88 180 | 85 700 | 92 000 | 99 280 | 89 230 | 87 200 | 93 000 | 100 170 | 90 280 | 88 700 |
| " | " | 87 555 | 85 300 | " | " | 88 605 | 86 800 | " | " | 89 655 | 88 300 | " | " | 90 705 | 89 800 |
| " | " | 87 980 | 86 300 | " | " | 89 030 | 87 800 | " | " | 90 080 | 89 300 | " | " | 91 130 | 90 800 |
| " | " | 88 405 | 87 200 | " | " | 89 455 | 88 700 | " | " | 90 505 | 90 200 | " | " | 91 555 | 91 700 |
| " | " | 88 830 | 88 000 | " | " | 89 880 | 89 500 | " | " | 90 930 | 91 000 | " | " | 91 980 | 92 500 |
| " | " | 89 255 | 88 700 | " | " | 90 305 | 90 200 | " | " | 91 355 | 91 700 | " | " | 92 405 | 93 200 |
| " | " | 89 680 | 89 300 | " | " | 90 730 | 90 800 | " | " | 91 780 | 92 300 | " | " | 92 830 | 93 800 |
| " | " | 90 105 | 89 800 | " | " | 91 155 | 91 300 | " | " | 92 205 | 92 800 | " | " | 93 255 | 94 300 |
| " | " | 90 530 | 90 300 | " | " | 91 580 | 91 800 | " | " | 92 630 | 93 300 | " | " | 93 680 | 94 800 |
| 95 000 | 103 550 | 91 880 | 88 200 | 96 000 | 104 440 | 92 930 | 89 700 | 97 000 | 105 330 | 93 980 | 91 200 | 98 000 | 106 220 | 95 030 | 92 700 |
| " | " | 92 305 | 89 300 | " | " | 93 355 | 90 800 | " | " | 94 405 | 92 300 | " | " | 95 455 | 93 800 |
| " | " | 92 730 | 90 300 | " | " | 93 780 | 91 800 | " | " | 94 830 | 93 300 | " | " | 95 880 | 94 800 |
| " | " | 93 155 | 91 200 | " | " | 94 205 | 92 700 | " | " | 95 255 | 94 200 | " | " | 96 305 | 95 700 |
| " | " | 93 580 | 92 000 | " | " | 94 630 | 93 500 | " | " | 95 680 | 95 000 | " | " | 96 730 | 96 500 |
| " | " | 94 005 | 92 700 | " | " | 95 055 | 94 200 | " | " | 96 105 | 95 700 | " | " | 97 155 | 97 200 |
| " | " | 94 430 | 93 300 | " | " | 95 480 | 94 800 | " | " | 96 530 | 96 000 | " | " | 97 580 | 97 800 |
| " | " | 94 855 | 93 800 | " | " | 95 905 | 95 300 | " | " | 96 955 | 96 800 | " | " | 98 005 | 98 300 |
| " | " | 95 280 | 94 300 | " | " | 96 330 | 95 800 | " | " | 97 380 | 97 300 | " | " | 98 430 | 98 800 |
| 100 000 | 109 600 | 96 630 | 92 200 | 101 000 | 110 590 | 97 680 | 93 700 | 102 000 | 111 480 | 98 730 | 95 200 | 103 000 | 112 370 | 99 780 | 96 700 |
| " | " | 97 055 | 93 300 | " | " | 98 105 | 94 800 | " | " | 99 155 | 96 300 | " | " | 100 205 | 97 800 |
| " | " | 97 480 | 94 300 | " | " | 98 530 | 95 800 | " | " | 99 580 | 97 300 | " | " | 100 630 | 98 800 |
| " | " | 97 905 | 95 200 | " | " | 98 955 | 96 700 | " | " | 100 005 | 98 200 | " | " | 101 055 | 99 700 |
| " | " | 98 330 | 96 000 | " | " | 99 380 | 97 500 | " | " | 100 430 | 99 000 | " | " | 101 480 | 100 500 |
| " | " | 98 755 | 96 700 | " | " | 99 805 | 98 200 | " | " | 100 855 | 99 700 | " | " | 101 905 | 101 200 |
| " | " | 99 180 | 97 300 | " | " | 100 230 | 98 800 | " | " | 101 280 | 100 300 | " | " | 102 330 | 101 800 |
| " | " | 99 605 | 97 800 | " | " | 100 655 | 99 300 | " | " | 101 705 | 100 800 | " | " | 102 755 | 102 300 |
| " | " | 100 030 | 98 300 | " | " | 101 080 | 99 800 | " | " | 102 130 | 101 300 | " | " | 103 180 | 102 800 |
| 105 000 | 115 650 | 101 380 | 96 200 | 106 000 | 116 540 | 102 430 | 97 700 | 107 000 | 117 430 | 103 480 | 99 200 | 108 000 | 118 320 | 104 530 | 100 700 |
| " | " | 101 805 | 97 300 | " | " | 102 855 | 98 800 | " | " | 103 905 | 100 300 | " | " | 104 955 | 101 800 |
| " | " | 102 230 | 98 300 | " | " | 103 280 | 99 800 | " | " | 104 330 | 101 300 | " | " | 105 380 | 102 800 |
| " | " | 102 655 | 99 200 | " | " | 103 705 | 100 700 | " | " | 104 755 | 102 200 | " | " | 105 805 | 103 700 |
| " | " | 103 080 | 100 000 | " | " | 104 130 | 101 500 | " | " | 105 180 | 103 000 | " | " | 106 230 | 104 500 |
| " | " | 103 505 | 100 700 | " | " | 104 555 | 102 200 | " | " | 105 605 | 103 700 | " | " | 106 655 | 105 200 |
| " | " | 103 930 | 101 300 | " | " | 104 980 | 102 800 | " | " | 106 030 | 104 300 | " | " | 107 080 | 105 800 |
| " | " | 104 355 | 101 800 | " | " | 105 405 | 103 300 | " | " | 106 455 | 104 800 | " | " | 107 505 | 106 300 |
| " | " | 104 780 | 102 300 | " | " | 105 830 | 103 800 | " | " | 106 880 | 105 300 | " | " | 107 930 | 106 800 |

Value of Carbon:
- Cunningham ..... .01 C = 1000 lbs.
- Campbell, Acid .... .01 C = 1210 lbs.
- Campbell, Basic .. .01 C = 950 lbs.
- Webster ..... .01 C = 800 lbs.

to .60 inclusive, and each five points of manganese from .20 to .60 inclusive. The original investigations from which these values are derived were made on rolled structural steel of ordinary thickness and under .35 carbon. It should be borne in mind that for all material over 3-8 in. in thickness, whether forged or rolled, a reduction in the estimated ultimate strength must be made. The amount of this correction for thickness increases as the thickness of the rolled or forged piece increases. This may be found to be greater for the higher carbon steels than for the lower carbon steels on which the original investigation was based. Mr Webster.

Mr. Campbell stated in presenting his values that his formula is not expected to apply to steel containing over .35 carbon or special alloys, and "in steel containing from .30 to .50% of carbon, the value of the metalloids is fully as great as with the lower steel, while the presence of silicon in such metal in proportions greater than .15% seems to enhance the strengthening effect of carbon."

I do not claim that the estimated ultimate strength of this higher carbon steel will be as near the actual ultimate strength as in the case of lower carbon steel, but putting the values in this form will enable other investigators to construct new tables with working values for both acid and basic steel of higher carbon.

This table also shows that by Mr. Campbell's values the estimated ultimate strength for acid steel of .35% carbon and over are uniformly higher than the values for basic steel of the same composition. As the carbon increases this difference increases.

Applying the values of this table to the upper and lower limits of carbon in Dr. Dudley's specifications and the possible variations in phosphorus and manganese, the results shown in the following table are obtained:

| C. | P. | Mn. | CUNNINGHAM. | CAMPBELL. | | WEBSTER |
|---|---|---|---|---|---|---|
| | | | | Acid. | Basic. | |
| .35 | .02 | .40 | 77 000 lbs. | 82 730 lbs. | 76 180 lbs. | 75 500 lbs. |
| .35 | .07 | .60 | 82 000 " | 87 180 " | 83 130 " | 85 300 " |
| .50 | .02 | .40 | 92 000 " | 100 880 " | 90 430 " | 87 500 " |
| .50 | .07 | .60 | 97 000 " | 105 330 " | 97 380 " | 97 300 " |
| Greatest differences | | | 20 000 lbs. | 22 600 lbs. | 21 200 lbs. | 21 800 lbs. |

Mr. Webster. This shows a variation of 20 000 lbs. or over in the estimated ultimate strength by any of these methods, and a difference of 29 150 lbs. between Mr. Campbell's basic steel with .35% carbon and the lowest phosphorus and manganese, and his acid steel of .50% carbon and the highest phosphorus and manganese. These differences are more than double the differences referred to by Dr. Dudley; that is to say, from 10 000 lbs. to 15 000 lbs. would be accounted for by half the permissible variation in the chemical requirements of his specifications.

In the higher carbon steel the variations of finishing temperature in rolling or forging have more effect on the ultimate strength than in the lower carbon steels.

It has never been advocated to substitute the estimated ultimate strengths for the tension tests or other physical tests. The chemical composition of the steel is only half the story, the other half is the change in the physical properties of the steel due to the mechanical work of rolling or forging and the heat treatment. That is to say, physical tests should always be made on the steel as it is put in service, and if the ultimate strength is of vital importance, tension tests should always be made.

Other users of *high carbon* acid and basic open-hearth steel confirm Dr. Dudley's statement, that acid steel gives higher ultimate strength for the same chemical composition than basic steel.

Mr. Dudley. CHARLES B. DUDLEY (by letter).—Since the meeting we have obtained the following tests of basic open-hearth steel, which are offered as a further contribution to our knowledge on this subject:

| | | | |
|---|---|---|---|
| Carbon | 0.344% | 0.413% | 0.525% |
| Manganese | 0.310% | 0.338% | 0.317% |
| Phosphorus | 0.013% | 0.019% | 0.023% |
| Silicon | 0.012% | 0.013% | 0.026% |
| Sulphur | 0.014% | 0.019% | 0.020% |
| Tensile strength | 63 722 lbs. | 70 875 lbs. | 77,998 lbs. |
| Elongation | 22.5% | 21.1% | 17.5% |

The above results were obtained from a test of axle steels made in the open-hearth furnace by the basic process. There were three grades of steel, as is noted, there being eight axles of each grade. Each end of each axle was cut up into test pieces, two

test pieces being taken from each end, making thirty-two test pieces in all; that is, the average above given is the average of thirty-two test pieces for each grade of steel. One-half the test pieces were taken from as near the circumference of the axle as possible, and the other half were taken so that the center of the test pieces was on a radius 40% of the distance from the centre to the circumference. The reduced section of the test piece was one inch diameter and the elongation was measured over eight inches. Two of the axles of each grade of steel had slices about an inch thick cut off after the eighteen inches required for test pieces had been taken, and from these slices borings were taken for analysis, the center of the drill taking the borings, starting in a prick punch mark on a radius 40% of the distance from the center to the circumference, as is required by the axle specifications of the Pennsylvania Railroad. The analysis given is the average of the figures given by these two lots of borings, each sample being analyzed separately. It will be noted that the actual figures obtained are quite widely different from the figures given in what is apparently the basic steels in Mr. Webster's table. It is evident that considerably more study is needed on this subject, and if our tests are to be relied upon at all, a revision of the factors used in calculating strengths of basic steel would seem to be in order. Mr. Dudley.

It has already been noticed several times in the above discussion, especially by Mr. Campbell and Mr. Webster, that the heat treatment of the steel, and the amount of work done on it, have an influence on the physical properties. I would simply suggest that possibly a part of the wide discrepancy between the results which we have actually obtained, and the results which would be expected from the chemical composition, may be due to the fact that our test bars were cut out of axles, no heating or forging having been done on the steel in the preparation of the test bars. Possibly the samples used by Mr. Webster, Mr. Campbell and others, in obtaining their factors, may have been differently treated, than was the steel in the axles which we have tested.

H. V. WILLE (by letter).—Our results at the Baldwin Locomotive Works agree with Dr. Dudley's in pointing to the fact that the same carbon in basic steel seems to result in a lower Mr. Wille.

Mr. Wille.

tensile strength than for acid steel. We gave this fact or theory a practical recognition in 1897 by lowering our tensile requirements from 80 000 to 75 000 lbs. We preferred to do this rather than to prescribe a high maximum carbon limit, because we also noticed that the higher carbon basic steels were more sensitive to any change in heat treatment than the lower carbon acid steel of the same tensile strength. In other words, the hardening effect did not vary with the tensile strength, but with the carbon content.

Application of the various methods of calculating the tensile strength would lead to the conclusion that the lower tensile strength of the basic steel is not due to the fact that this steel contains less phosphorus than acid steel, in fact as our specifications have a maximum phosphorus limit of .05, there is not a marked difference between the amount of phosphorus in the acid and basic steels.

I have taken at random from our records 400 tests of steel blooms equally divided between four manufacturers, two of which made acid and two basic steels, and from the analysis calculated the tensile strengths by the methods proposed by Messrs. Webster, Campbell and Cunningham. The averages of these results are shown in the following table:

| | Actual. | Webster. | Campbell. | Cunningham. |
|---|---|---|---|---|
| Manufacturer A, acid | 82636 | 88766 | 95929 | 89494 |
| Manufacturer B, acid | 87850 | 85630 | 93120 | 86270 |
| Manufacturer C, basic | 79971 | 85954 | 87490 | 87235 |
| Manufacturer D, basic | 78010 | 88920 | 90270 | 90760 |

It will be noticed:

First. That there is a much greater difference between the calculated and actual tensile strength in the basic than in the acid steels.

Second. That the variations are not uniform.

The results obtained by Mr. Webster's method of calculations are nearest those actually obtained. In the acid steels, the calculated results are 2 100 lbs. high in one instance and 1 300 lbs. low in another, but in the two makes of basic steel the results are about 6 000 and 11 000 lbs. high. It is thus seen that the formula devised by Mr. Webster can be applied with considerable accuracy to acid steel, but it gives results much too high for

basic steel, and further demonstrates that there is a difference in the tensile strength of acid and basic steel of the same chemical composition. Mr. Wille.

The question now arises, can this condition be due to the essential differences in the process of manufacture, that is, to the fact that the acid process is primarily a melting operation whereas the basic is a combined chemical and melting operation? If this were the case there would probably be less variation between the different makes of the steel made by the same process. The indications are that the variations will be found to be due to some other causes. It may be that the method of recarbonization may exercise a profound influence on the physical properties of the steel. I regret that I have not complete data as to the methods pursued by the four manufacturers, the tests of whose steel have been quoted, but it may be stated in general that the acid steel is recarbonized either by making additions in the furnace or by running the bath to the carbon desired, whereas the carbon in the basic steel is added in the ladle.

# STEEL RIVETS.

By Gaetano Lanza.

A considerable amount of investigation has been carried on in the engineering laboratories of the Massachusetts Institute of Technology, having for its object, to determine the ultimate compression on the bearing surface, in the case of riveted joints made in mild steel plates, with wrought iron rivets, each joint containing at least three, four or five rivets.

Inasmuch as steel rivets are coming to be very extensively used, it is plain that they should be included in any such investigation.

Before undertaking the more complex problem of the compression on the bearing surface, however, we need to know the quality of the steel used in making rivets, and to what extent the quality varies.

A preliminary research, therefore, becomes necessary, in which we shall seek to ascertain the physical and other properties of the steel rivet material now sold in the market, and those of the rivets themselves; and also how these properties compare with those of the wrought iron rivets that have heretofore been used; and how they compare with the standard specifications of this Society. It is the object of this paper to give an account of a partial investigation of these preliminary questions.

A perusal of the extensive series of tests of riveted joints made at Watertown Arsenal, shows that the wrought iron rivet metal used in those tests had a tensile strength varying from about 52 000 to 59 000 pounds per square inch and a shearing strength, as shown by tests of wide joints, varying from about 35 500 to 46 000 pounds per square inch, with an average of about 39 000.

On the other hand, the shearing strength per square inch, of the small number of steel rivets used in these Watertown Arsenal tests varied from about 51 000 to 65 000; and it is evident, therefore, that these steel rivets were made of a harder metal than would be tolerated at the present day.

In order to avoid any possible duplication of work already performed by others, a certain amount of correspondence was carried on with some gentlemen who, in the opinion of the writer, would probably know what had already been done; but, while more or less light was thus thrown upon certain side issues, the correspondence failed to elicit the information desired.

Some preliminary correspondence was then carried on with three makers of steel rivets, viz: the American Iron and Steel Manufacturing Company, the Champion Rivet Company, and Messrs. Hoopes & Townsend, the object being to ascertain how many and what different grades of steel rivets were made by each one, and their standard sizes.

After this information was received, they were each asked to furnish as many $3\frac{1}{4}$ inch standard rivets as could be made from two bars 12 to 15 feet long (preferably from the same bloom) of each of the following sizes: $\frac{7}{16}''$, $\frac{11}{16}''$, $\frac{3}{4}''$, $\frac{13}{16}''$, $\frac{7}{8}''$, $1''$, the last 20 to 24 inches of each bar being reserved as a test sample, the rivets made from each bar, with the test sample, to be put in separate bags, and shipped to us. Each firm was asked to omit any sizes for which they had not the dies.

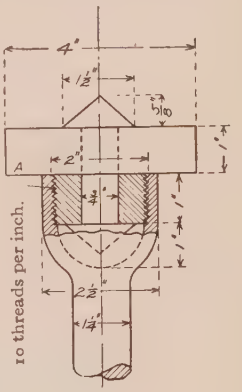



FIG. 1

The rivets and rods having been received, the plan of the investigation was formulated as follows, viz:

1. The tensile properties of the rods were to be determined including (a) the tensile strength, (b) the limit of elasticity, (c) the modulus of elasticity, (d) the contraction of area at fracture, (e) the ultimate elongation in a gauged length of eight inches.

2. Suitable bending tests.

3. The determination of the chemical composition of the steel.

4. The tensile strength of the rivets.

5. The shearing strength of the rivets, as determined from tests of double shear joints with only one rivet in each joint.

6. The shearing strength of the rivets, as determined from tests of double shear joints with three, four or five rivets in each joint.

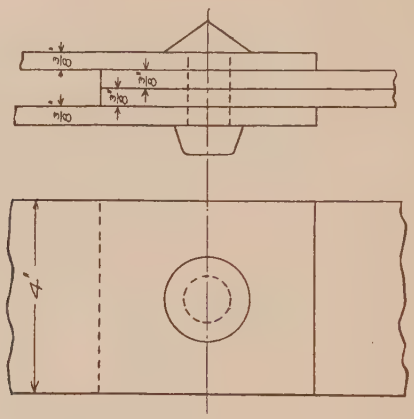


FIG. 2.

| JOINT. | A | B | C | D | E |
|---|---|---|---|---|---|
| Diameter of rivet..... | $1\frac{1}{16}''$ | $1\frac{5}{16}''$ | $\frac{7}{8}''$ | $1\frac{3}{16}''$ | $\frac{3}{4}''$ |
| Lap ..... | $2\frac{5}{8}''$ | $2\frac{3}{8}''$ | $2\frac{1}{4}''$ | $2\frac{1}{8}''$ | $2''$ |

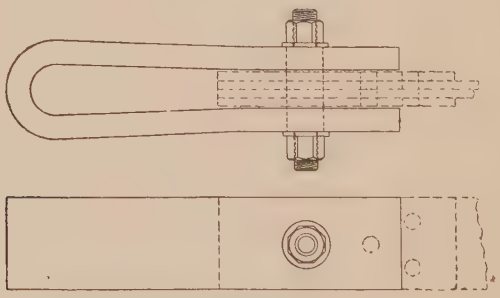


FIG. 3.

7. To these were added, later, an examination of the microstructure of the steel.

After a few of the rods had already been tested in the laboratory, the work was taken up and carried on by Mr. J. D. Ireland, a student of the graduating class in Mechanical Engineering, for his thesis.

Most of the work performed under the headings 1, 5, 6 and 7 was carried on by Mr. Ireland, No. 7, in the metallographical laboratory of the Institute, and the others in the laboratory of Applied Mechanics of the Institute; while No. 3 was performed by students of the Chemical Department under the direction of Professor Henry Fay.

Although the work indicated in the formulated plan has not all been completed, the results thus far obtained seem to

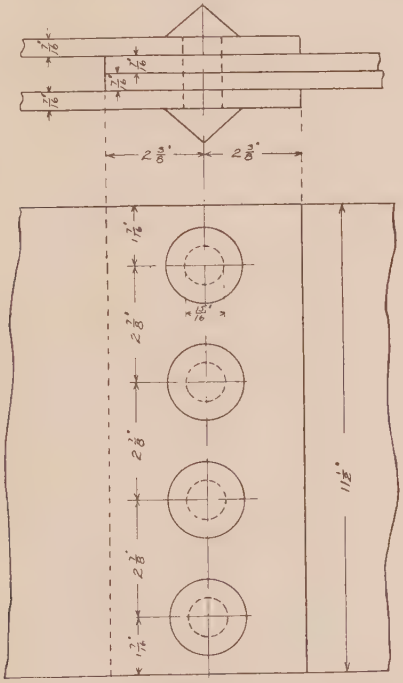


FIG. 4.

me to be of sufficient interest to warrant their presentation in this paper. While only a few bending tests, and no tensile tests of the rivets themselves have been performed, the results of the other tests will be given in tabulated form after a brief explanation of the manner of conducting them.

No. 1. These tests were made in the usual manner by means of the testing machine of the laboratory, and require no description.

No. 4. The manner of holding the rivet for the purpose of determining its tensile strength is shown in Fig. 1, and need not be further described.

No. 5. The form of joints used and of the holders are shown in Figs. 2 and 3. The joints were in each case so proportioned that the rivet should fail by shearing; care was taken also to

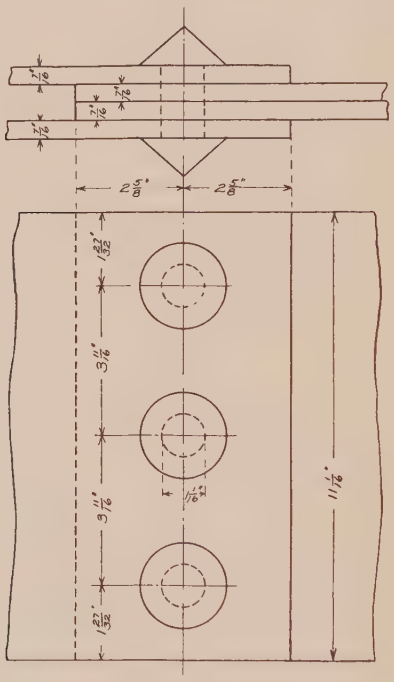



FIG. 5.

make the compression on the bearing surface as small as possible; it reached 95 000 in only one case and was generally below 80 000.

No. 6. Only two tests were made under this head, the joints tested being shown in Figs. 4 and 5, and the holders in Fig. 6; hence it is not fair to draw any conclusions from them.

No. 7. For the metallographic work samples were taken from the unstrained ends of the rods.

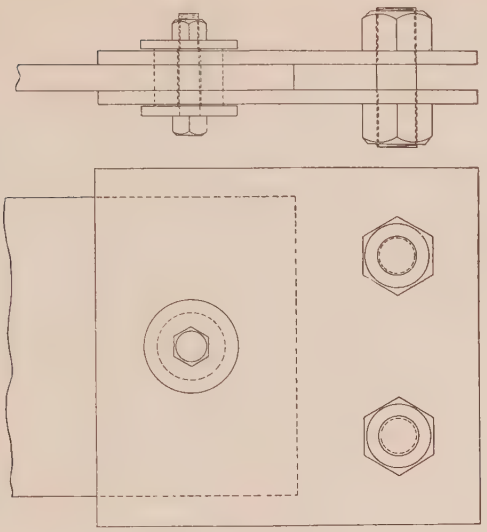


FIG 6.

The tables of results follow:

TABLE I.—TESTS ON RODS.

| Diameter of Rod. Inches. | Original Area of Section. | Gauged Length. Inches. | Ultimate Strength. Lbs. Per Sq. In | Elastic Limit. Lbs. Per Sq. In. | Contraction of Area at Fracture. Per Cent. | Ultimate Elongation. Per Cent. |
|---|---|---|---|---|---|---|
| .67 | .353 | 8 | 51 760 | 24 100 | 67.85 | 33.9 |
| .72 | .406 | 8 | 46 180 | 22 160 | 77.67 | 35.9 |
| .78 | .478 | 10 | 50 000 | 23 000 | 74.40 | 37.5 |
| .86 | .581 | 8 | 49 800 | 21 000 | 70.20 | 35.0 |
| .98 | .754 | 10 | 49 410 | 19 900 | 66.16 | 34.7 |
| .67 | .353 | 8 | 49 750 | 21 320 | 72.73 | 34.5 |
| .73 | .419 | 8 | 47 700 | 23 900 | 49.26 | 27.5 |
| .78 | .478 | 10 | 50 300 | 20 930 | 69.60 | 35.0 |
| .85 | .568 | 8 | 50 620 | 26 440 | 66.76 | 35.9 |
| .98 | .754 | 8 | 49 930 | 25 200 | 66.16 | 37.3 |
| .66 | .339 | 8 | 49 650 | 25 070 | 67.78 | 28.0 |
| .79 | .490 | 8 | 45 400 | 22 450 | 71.75 | 35.0 |
| .83 | .538 | 8 | 45 010 | 29 740 | 66.36 | 33.3 |

TABLE II.—TESTS ON JOINTS—RIVETS IN DOUBLE SHEAR.

| No. of Rivet in Joint. | Original Rivet Diameter. | Driven Rivet Diameter. | Area of Driven Rivet—Sq. In. | Length of the Protruding Portion of Rivet for Forming Head and Filling Hole. | Color of Rivet When Driven. | Pressure in Tons. | Time Pressure Applied Sec. | Lap in Terms of Rivet Diameter. | Lap in Inches. | Thickness of Double Plates. | Max. Load on Machine—Lbs. | Breaking Load Per Sq. In. of Original Driven Area of Rivet. | Area of Sheared Section. | Reduction of Area of Sheared Section in Per Cent of Original Driven Rivet Area. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | $1\frac{1}{16}$″ | $\frac{3}{4}$″ | .44 | $1\frac{1}{4}$″ | Bright red. | 21 | 10 | $2\frac{2}{3}$ | 2″ | $\frac{3}{4}$″ | 41 800 | 47 285 | .36″ | 18 |
| 1 | $\frac{3}{4}$″ | $1\frac{3}{16}$″ | .52 | $1\frac{1}{4}$″ | Between bright and med. red. | 24 | 15 | $2\frac{8}{13}$ | $2\frac{1}{8}$″ | $\frac{3}{4}$″ | 51 750 | 49 949 | .38″ | 27 |
| 1 | $1\frac{3}{16}$″ | $\frac{7}{8}$″ | .60 | $1\frac{1}{4}$″ | Between bright and med. red. | 27 | 14 | $2\frac{8}{14}$ | $2\frac{1}{4}$″ | $\frac{3}{4}$″ | 56 000 | 46 600 | .48″ | 20 |
| 1 | $\frac{7}{8}$″ | $1\frac{5}{16}$″ | .69 | $1\frac{1}{4}$″ | Between bright and med. red. | 30 | 12 | $2\frac{8}{15}$ | $2\frac{3}{8}$″ | $\frac{3}{4}$″ | 63 000 | 45 630 | .52″ | 25 |
| 1 | 1″ | $1\frac{1}{16}$″ | .89 | $1\frac{1}{4}$″ | Bright red. | 33 | 14 | $2\frac{8}{17}$ | $2\frac{5}{8}$″ | $\frac{3}{4}$″ | 78 000 | 43 980 | .64″ | 28 |
| 1 | $1\frac{1}{16}$″ | $\frac{3}{4}$″ | .44 | $1\frac{1}{4}$″ | Medium red. | 21 | 13 | $2\frac{2}{3}$ | 2″ | $\frac{3}{4}$″ | 40 000 | 45 260 | .36″ | 18 |
| 1 | $1\frac{1}{16}$″ | $\frac{3}{4}$″ | .44 | $1\frac{1}{4}$″ | Bright red. | 21 | 13 | $2\frac{2}{3}$ | 2″ | $\frac{3}{4}$″ | 41 000 | 46 380 | .36″ | 18 |
| 1 | $\frac{3}{4}$″ | $1\frac{3}{16}$″ | .52 | $1\frac{1}{4}$″ | Dull bright red. | 24 | 13 | $2\frac{8}{13}$ | $2\frac{1}{8}$″ | $\frac{3}{4}$″ | 39 500 | 32 840 | .44″ | 15 |
| 1 | $\frac{7}{8}$″ | $1\frac{5}{16}$″ | .69 | $1\frac{1}{4}$″ | Bright red. | 30 | 13 | $2\frac{8}{15}$ | $2\frac{3}{8}$″ | $\frac{3}{4}$″ | 53 000 | 38 390 | .59″ | 15 |
| 1 | 1″ | $1\frac{1}{16}$″ | .89 | $1\frac{1}{4}$″ | Medium red. | 33 | 11 | $2\frac{8}{17}$ | $2\frac{5}{8}$″ | $\frac{3}{4}$″ | 79 000 | 44 552 | .75″ | 16 |
| 1 | $\frac{3}{4}$″ | $1\frac{3}{16}$″ | .52 | $1\frac{1}{4}$″ | Medium red. | 24 | 15 | $2\frac{8}{13}$ | $2\frac{1}{8}$″ | $\frac{3}{4}$″ | 45 000 | 43 390 | .44″ | 15 |
| 1 | $1\frac{3}{16}$″ | $\frac{7}{8}$″ | .60 | $1\frac{1}{4}$″ | Medium red. | 27 | 12 | $2\frac{8}{14}$ | $2\frac{1}{4}$″ | $\frac{3}{4}$″ | 54 000 | 44 900 | .48″ | 20 |
| 1 | $\frac{7}{8}$″ | $1\frac{5}{16}$″ | .69 | $1\frac{1}{4}$″ | Medium red. | 30 | 14 | $2\frac{8}{15}$ | $2\frac{3}{8}$″ | $\frac{3}{4}$″ | 60 800 | 44 040 | .52″ | 25 |
| 1 | 1″ | $1\frac{1}{16}$″ | .89 | $1\frac{1}{4}$″ | Medium red. | 33 | 16 | $2\frac{8}{17}$ | $2\frac{5}{8}$″ | $\frac{3}{4}$″ | 80 000 | 45 120 | .61″ | 31 |
| 4 | $\frac{7}{8}$″ | $1\frac{5}{16}$″ | .69 | $1\frac{1}{4}$″ | Bright red. | 30 | 13 | $2\frac{8}{15}$″ | $2\frac{3}{8}$″ | $\frac{7}{8}$″ | 265 000 | 46 360 | .51″ | 26 |
| 3 | 1″ | $1\frac{1}{16}$″ | .89 | $1\frac{1}{4}$″ | Bright red. | 33 | 14 | $2\frac{8}{17}$″ | $2\frac{5}{8}$″ | $\frac{7}{8}$″ | 241 000 | 49 140 | .65″ | 27 |

In the light of these tests we may observe that—

1. The rivet material furnished by all the three makers appears to conform to the specifications of this Society.

2. The shearing strength of the rivets, as determined from tests of joints with only one rivet, is, in general, nearly as great as the tensile strength of the rivet metal.

3. How much lower the shearing strength is when determined from the tests of wide joints cannot be inferred from only two tests.

4. In the light of the chemical work thus far completed, I infer that the rivet material furnished was basic steel.

5. Samples taken from the unstrained ends of the rods, and examined under the microscope showed a granular structure.

# THE ETHICS OF TESTING.

By Paul Kreuzpointner.

It may seem strange to connect ethics with the science of metallurgy and testing. However, since ethics enters into every action which we perform, as a matter of duty towards ourselves, our employers, fellow citizens or the state, it is unavoidable that the result of our daily work is being influenced by the higher or lower ethical standard of the man who is charged with the duty and confidence of testing materials of construction.

Thus, the results of testing represent the sum total of skill, knowledge and intelligence of those performing the work, plus the degree of sense of duty, integrity, sincerity, honesty and reliability they possess, or in short, their standard of ethics. The work of devising specifications and testing the materials thus specified, are commercial transactions, and since moral conduct, that is, ethics, enters so largely into all commercial transactions, it is not too much to say that the science of conduct is first cousin to the science of applied metallurgy.

The care exercised in testing is not only a matter of skill, but also a matter of good will, and the latter is purely an ethical function. Frequently we hear complaints of a prevailing low commercial spirit of individuals or concerns, as injurious to the reputation of community. On the other hand, communities are proud in the possession of citizens whose word can be trusted implicitly and whose high ethical standard is cited as an example worthy to be followed. And it must ever be so, because civilized life could not be maintained, or order and confidence could not prevail, property would not be secure and obligations would not be fulfilled, if ethics were not an integral part of man's daily and hourly action.

Hence, in order that the work of devising specifications and the testing of materials of construction, have a reliable commercial value to both producer and consumer, we must follow faithfully the teachings of science, but in order to perform work faithfully, we must be able to command a high sense of duty, and a high sense of duty means a high ethical standard. This

ought to put the work of testing upon a higher plane than mere mechanical skill, and it is to be regretted that the factor of ethics receives so little recognition in estimating the commercial value of the man's work who does the testing.

In work requiring purely mechanical skill, the result can be measured by rule and compass, and these are tangible quantities. In testing, however, the results must be accepted in good faith, and the man's integrity becomes a leading factor of the reliability of the work performed, because no one can measure or caliper the result. Thus, clearly, testing is largely a question of man's ethics. However scientifically correct and commercially acceptable specifications may be, their intrinsic value to the consumer is largely determined by the degree of reliability with which the final work of testing is performed.

Because of this factor of ethics playing such a prominent part in determining the commercial value of the science of testing, it is neither scientifically correct nor commercially judicious to consider the work of testing of low commercial value, to be performed by any man of average intelligence, who is willing to accept low pay. And, the closer the minimum and maximum limits of the specifications are drawn, the more complicated the work and method of testing is, the more this holds good. The properties and qualities of metals are not such fixed, unalterable quantities as not to be susceptible to a greater or less change, according to the will, knowledge or ignorance of the operator of the testing machine, or those in charge of testing, and therefore no amount of mechanical or automatic devices will make up for any possible absence of ethics from a testing room.

The degree of the viscosity of a metal determines its value for a given purpose, and this very peculiar and very sensitive property of metals, the viscosity, is easily influenced by methods of preparation of test pieces, by shape of test section and manner of testing; hence, leaving out errors in measurements, in weighing and in the machines, there is ample scope in testing for the application and infusion into the work of a large measure of ethics. There is another aspect of this question of national importance, if you please, and this is the degree of confidence our work of testing and devising specifications commands abroad now and in the future.

Our foreign competitors are very well versed in the properties and qualities of materials of construction, and the way these properties and qualities are influenced by the proper or improper treatment of these materials, hence any shortcomings on our part, in the knowledge of the properties and qualities of materials, or mistaken application of that knowledge, will be quickly noted and treated accordingly, to our disadvantage. Hence, a proper blending of the science of metallurgy and the science of ethics assumes the dignity of a patriotic duty well performed.

# STANDARD CEMENT SPECIFICATIONS.

By R. W. Lesley.

Of the making of specifications, there is no end. This, at least, is the experience of the cement manufacturer, whose material, instead of being tested according to lines thoroughly well settled, as in the iron and steel business and tested at the place of manufacture, as is the practice in other businesses, is subjected to endless variations of tests, which are frequently made thousands of miles away from the point at which the goods are produced. The material tested is not the material that leaves the works, as its form and character are changed by making it into briquettes, test pieces, etc., neat and with sand, and it is subjected to all sorts of manipulations, so that the results obtained frequently vary materially from those made under similar conditions at the place of manufacture. Consequently, there is no industry worse off, for definite specifications, than the cement industry, and none has suffered more than that industry from the lack thereof.

It is with pleasure, therefore, that I am here to address an Association the purpose of which is the standardizing of testing and methods of testing, and it is my pleasure in addressing it to state, in a very general and brief way, some of the disadvantages that the cement industry has labored under during the formative period of tests which has been going on in this country, and some of the many advantages that would accrue as well to the manufacturer, the user, and the engineer and architect, by the adoption of standard methods and the correlation of different standards and specifications adopted by the various classes of engineers and consumers using the material.

In my office, I have a large fireproof safe containing many volumes of filing cases, solely devoted to the embalming in fireproof receptacles of specifications that have come to me at various times during my business career. These specifications are of all kinds and of all characters, mostly good, but some few bad and indifferent, containing elements of every imaginable description,

both as to the requirements and as to methods of handling the material in arriving at the requirements suggested. This is the mere literary side of cement specifications that I have in my office.

The practical side of this business is at our laboratory, at our works. This laboratory, quite a large room, say, 20 x 20 feet, a few years ago was entirely papered on its four walls with cement specifications. One wall was devoted to the specifications of the army engineers of the United States Government, well considered, well thought out and well reasoned specifications, all of them. Another side was almost covered with specifications of the United States navy engineers of the Bureau of Yards and Docks and other bureaus of the same department; all specifications thoroughly thought out, well planned, well conceived. At the other end of the room the pattern varied somewhat, and there were gathered the requirements as to quality and the methods of manipulation of cement testing to be done by the various railroads of the country. They were more concise, more brief and more to the point, but all of them intelligent, able and well drawn. On the other side of the room, surrounding the door, were grouped a more general gathering of these interesting documents, and embraced what the city engineers all over the country and the engineers of large municipal works in the United States thought should be required of cement, and how it should be handled in arriving at these requirements. Needless to say, this series of patterns embraced a larger variety and covered a larger area of territory, and, like the others, represented careful, hard work, and well conceived and well balanced requirements.

Individually, almost any one of the specifications on the four sides of that room, could be picked out at random, as a fair, reasonable specification of what Portland Cement should be. Almost any one of them could be used with advantage and with success and, with few exceptions, any one of them could be carefully scrutinized and nothing seriously ill-judged or unbalanced could be found within its lines.

It is a remarkable tribute to the engineering ability of our country that when all these specifications which covered the four sides of a room and running into the hundreds in number, are gone over carefully, that there are so few which will not stand the test of careful examination and scrutiny, and the conclusion

is to be clearly drawn from the facts above stated, that it is not so difficult for intelligent engineers to make a specification containing the proper requirements for fineness, tensile strain, time of setting, constancy of volume and specific gravity and for proper manipulation of the several tests described. Yet with all these facts before us, it is actually possible that within a single year a single large cement establishment like our own receives no less than 100 to 150 different kinds of specifications, varying in most all their elements.

Taking an assortment of specifications at random and in examining all of them to see just how they did vary, I find that taking the four great classes, the army, the navy, the railroads and the various cities, there were in some:

Twenty-five specifications of various cities of the United States, 7 variations in the item of fineness alone, 14 variations in the requirements of tensile stress, and 6 variations out of 9 requirements in reference to setting-time. In the specifications of the army, some 68 in number, I find some 14 variations in tensile stress, 11 variations in fineness, 7 variations in time of setting, and 4 in constancy of volume. In the specifications of the navy, some 7 in number, there were 3 variations in fineness, 5 in tensile strength, with no variation in the time of setting. Among the railroads, of which 8 specifications were found, there were 5 variations in fineness, 5 variations in tensile stress and 3 variations in the time of setting.

As to tensile strength alone, the variations in this branch of testing are endless. It would be useless to take up the time of this meeting by discussing all the variations in this one element of testing as applied to neat and sand tests. It would only give a series of figures which would show how impossible it is for the manufacturer to regulate his product to meet all the requirements of the various specifications that are presented to him to bid upon.

Analyzing, however, for purposes of illustration only, a single one of the tests referred to as the "hot test" or what is known as the boiling or accelerated test a remarkable state of affairs is shown: These tests first came in 1889, and were experimented on and recommended by Tetmajer, Maclay and others.

They are described by Professor Baker, in the *Brickbuilder* for February, 1899, and also by Professor Gary in "Proceedings

Royal Testing Station," Berlin, 1899, and Professor Spalding in his book on "Hydraulic Cement," page 189, and are:

Warm water tests of Faija, 100° to 115° F.
Hot water tests of Maclay, 195° to 200° F.
Boiling water tests of Tetmajer, 212° and over.
Kiln tests of Tetmajer, 166° to 248° F.
Flame tests of Heintzel, Bunsen burner.
Chloride of lime tests of Candlot.
Boiling tests of Michaelis, 212° and over.
Compressed cake tests of Prussing.
High pressure steam tests of Erdmenger.
Caliper apparatus of Baushinger.

All these accelerated tests are in every way different from each other, hot water, steam, hot air flame and the heat of an oven all being used. No two agree in temperature of water or air, and few agree in form of specimen pat, time after gaging they are to stand the test, or time of duration of the accelerated test.

So on the whole, taking the matter of accelerated tests in its entirety, there is no such well-defined uniform method proposed that like the other elements of cement testing, could be put in use by the ordinary tester with ordinary methods.

From a mere recital of these tests, nearly all of which appear in some of the specifications offered to bid upon, it can be seen what a variety of elements is presented to the manufacturer. Eliminating all these various requirements of accelerated tests, except a *single* one, namely, the boiling or hot water test, and analyzing some 35 tests on this one element alone, gathered from 200 to 300 American specifications, it can be seen what the manufacturer has to meet. In every case pats or briquettes are required to be made and submitted to this boiling or hot water test.

Out of these 35:
Ten go in water after 24 hours.
Fourteen go in water after no specified time.
Eleven go in water after setting.

Out of these 35:
Six go in steam for 3 hours.
Fourteen go in hot water for no specified time.
One goes in hot water for 3 hours.
Two go in hot water for 30 minutes.

One goes in hot water from 24 to 36 hours.
Eight go in hot water for 24 hours.
One goes in hot water for 10 hours.
Two go in hot water from 24 to 48 hours.

Out of these 35:

Twenty-one go in boiling water.
Four go in water at a temperature of 212° F.
Six go in water at a temperature of 176° F.
One goes in water at a temperature of 170° F.
One goes in water at a temperature of 100°.

Out of these 35:

Thirteen are to boil for a time not specified.
Two are to boil 30 minutes.
One is to boil 3 hours.
One is to boil 24 to 36 hours.
Three are to boil 24 to 48 hours.
One is to boil 10 hours.
Five are to boil 24 hours.
One is to steam 3 hours, then to be placed in hot water 48 hours and to boil 1 hour.
One is to steam 3 hours, and to boil 48 hours.
Four are to steam 3 hours and then to be placed in hot water 48 hours.
Two are to be placed in hot water 176° F. for 24 hours.
One is to be placed in hot water, 110° to 115° F., for 24 hours.

From this it can readily be seen that, from the manufacturer's standpoint, not only the pat, but the manufacturer himself, is in "hot water" for all sorts of indefinite periods of time.

Now, imagine for just one moment the manufacturer's plight while trying to make a cement during the course of the short period in which these specifications were received, which would meet all these different requirements and conditions, and then you can see at a glance how important it is, not only that there should be a standardizing of methods, but that there should be a standardization of specifications. I am happy to say that two departments of our Government have already taken this step, and where one wall was formerly covered with the specifications of the United States army, that wall is a blank, with the single

exception of the specifications (see *The Engineering Record* of September 14 and 21, 1901) recommended by the Army Board, composed of Major W. L. Marshall, Major Smith L. Leach and Captain Spencer Cosby, and approved by the United States Engineer Corps. This shows the result of the good work of a very capable force of engineers.

The other wall, formerly having as its decoration the productions of the engineers of the navy, is to-day practically a blank space, with a single ornament in its center, namely, the small frame containing the specifications of Rear Admiral M. T. Endicott, U. S. N., giving the requirements for all the work of the United States navy (see *The Engineering Record* of February 4, 1899). These two Government specifications are not quite alike, but in many of their elements are at least within "shooting range" and it is possible to meet them, without performing the gymnastic feats in manufacturing which were requisite to meet the requirements of the many varieties of their predecessors on our walls.

We still have with us the railroads and the municipalities. The railroads are taking a step in the same direction as these two branches of the Government, and while there are yet no specifications formally adopted by all the railroads in this part of the country, the last convention of the American Railway Engineering and Maintenance of Way Association adopted tentative specifications (see *The Engineering Record* of April 12, 1902) which represent much good work and are in the line of standardization in that department.

The cities and commonwealths of the country, in the execution of their large work, have not yet arrived at the standardization of specifications, but we have hopes that it will come next.

Now what I want to make clear, after having gone through this underbrush of discussion, is that this Society of ours has within its hands a golden opportunity in the field of cement specifications. The army, the navy and the railroads have all gone forward in the proper lines and are practically committed to forms of specifications embracing requirements for the various elements and properties and for methods of manipulation giving reliable results by the respective tests suggested. The American Society of Civil Engineers has appointed a committee to determine upon the proper manipulations of tests of cement, and this

committee, several of whose distinguished members are with us to-night, has been laboring with this subject for several years, and I understand will shortly report.

Now our Society cannot, with loyalty to itself or regard for its fellow society and the engineers of the Government and the railroads, undertake to undo or criticize the work that these bodies have done. It cannot with propriety adopt new methods of manipulation while so distinguished a body as the American Society of Civil Engineers has this subject under consideration. It cannot of its own motion set up new standards and new requirements, while the army, the navy and the railroads have come to conclusions of their own on this subject. But this Society of ours, international in its features, can at a time when the export trade in this country in cement is increasing at the rate of 500% a year, take steps in co-operation with army and navy engineers and the American Society of Civil Engineers, and in co-operation with scientific bodies abroad, to arrive at some method by which a correlation of the specifications of this country and Europe may be reached and results obtained whereby cement manufactured at any point of the world may be tested under similar methods of manipulation and subjected to similar tests at any point where it may be delivered. This cannot be done by independent action, but it must be done by suggestive and persuasive action, and with the support and co-operation of the bodies who have preceded us in the field and have carried out independent investigations on their own lines. It can be best done by the organization, under the auspices of this body, of a cement section, which shall have among its membership representative engineers of the principal consumers of cement, distinguished testing engineers representative of the various well-known testing laboratories, and also representatives of the leading manufacturers. Such a section, comprising in itself representatives of the consumer, the producer and the inspecting engineer, would soon arrive at proper methods for the accomplishment of the object and the work would redound to the credit of all concerned, as well as to the Society of which they are members.

---

NOTE.—The joint discussion of this paper and the succeeding one on "The Advantages of Uniformity in Specifications for Cement and Methods of Testing," by George S. Webster, follows on pp. 133–138.

# THE ADVANTAGES OF UNIFORMITY IN SPECIFICATIONS FOR CEMENT AND METHODS OF TESTING.

By George S. Webster.

The attainment of a Standard Specification is greatly desired by all those called upon to use cement in works of construction; indeed, this matter is of such importance to the manufacturer and consumer that both are concentrating their efforts in endeavoring to accomplish this result.

Mr. Lesley has given us much interesting information in regard to the great variations existing at the present time in specifications for cement, these variations not only arising from differences of opinion between experts as to what constitutes good material, and from different methods of testing, but also arising in many instances from lack of knowledge or from inexperience. Some of the specifications call for high strength, others for low; some require quick and others slow setting; many require a finely ground product, to be slow setting, and to be low in sulphuric acid, which is difficult and often impossible to produce; others introduce chemical qualifications, which, if not submitted to an expert for approval, often impose very unnecessary restrictions on the manufacturer.

The difficulties under which the manufacturer labors are evident. His material must satisfy all of the specifications of his different contracts, and a single product can scarcely be controlled by subsequent treatment to meet these different requirements.

The advantages of uniform specifications to a manufacturer would be great. Instead of being obliged to meet irrational and sometimes impossible specifications, requiring a great variety of grades of material, he would be required to produce only one, or possibly two, grades, one quick and one slow setting. His work would be greatly simplified, which would not only reduce the probability of producing inferior material, but also tend to reduce the cost of production.

The advantages of uniformity in specifications, moreover, are by no means entirely in favor of the manufacturer. The

material supplied under a uniform specification would necessarily result in a production having more uniform properties and qualities. This material would be ground to a certain fineness, have a definite specific gravity, and develop a strength with less range of variation, when treated under definite conditions. Thus the consumer could depend upon the quality of his material better than he does now, and would consequently be able to use it more understandingly, by adopting more efficient methods of manipulation and more economical designs. He would also be able to establish the fact when failures in mortar and concrete occur from poor workmanship and improper manipulation of the materials, rather than from the inferior quality of the cement, and he would apply such remedies as would secure more durable and permanent structures. The investigation of cement in any one branch of construction would have a definite bearing on all cement construction and could be utilized in every line, whereas now they excite interest only in a general way.

Again, the consumer would not only save money through economies in manipulation and design, but his material would actually cost less, due to the reduction in cost to the manufacturer, resulting from the production of a standard article. The small consumer, also, not having sufficient appliances for accurate testing, would be more sure of the character of his material. Under the present system he is liable to get all the inferior products of the mill and the shipments which have been condemned on more important work.

## METHODS OF TESTING.

Every one connected with a laboratory for testing cement knows the great influence which details of seemingly minor importance exert upon the results. In the preceding part of this paper, it has been assumed that uniformity in specifications is equivalent to uniformity in the material itself, but this evidently would not be the case unless the specifications were based upon a standard method of testing. For instance, one laboratory might obtain a certain value on a seven day sand test and another might obtain a much greater value on exactly the same material. Therefore if the uniform specification called for a value intermediate between the two, the first laboratory

would require a higher testing cement than the second, even should their requirements be identical. In order for a uniform specification, therefore, to have any practical value, it must be based on a standard method of testing.

The principal reason that tests of cement show such variations in the results obtained by different operators, is that it is one of the few materials that is not tested entirely in the form in which it is manufactured and sold. Bars of iron and steel, bricks and wood, are tested not only in the form in which they are to be used, but also in the form in which they are produced and sold. Cement, on the contrary, is made in one form, tested in a second, and used in a third form.

The tests of cement may be divided into two classes: First, those which can be made with comparative accuracy; and second, those which are only relative, owing to the great influence which personal equation has upon the results.

The first class of tests are those made on the material as it is manufactured and sold, *i. e.*, specific gravity, fineness and chemical analysis. The second class includes those tests which are made on the material after it has been subjected to certain processes, and combined with other elements, and hence exists in a different form from that in which it was produced, *i. e.*, time of setting, tensile and compressive strength, and soundness.

The first class of tests are capable of standardization, providing the apparatus and materials used are made and handled with precision. The second class, on the other hand, are subject not only to variations in the material itself, but also to variations in the other elements used with this material, and to variations in the processes employed in combining them.

On account of the difficulty of procuring suitable apparatus for making tests, and of manipulating it with exactness, variations will be found to some extent to affect all tests.

The determination of specific gravity is probably less subject to variations due to differences in method, where reasonable care is exercised in its manipulation.

The test of fineness, on the other hand, is subject to variations on account of the difficulty of procuring standard sieves and of manipulating them with precision.

Chemical analyses are also subject to similar irregularities.

In the tests made on pastes of cement, the inaccuracies of manipulation enter with double force, on account of the introduction of other elements, and of the greater influence of the personal equation.

On account of these many possible sources of error, therefore, it is evident that every detail of manipulation, and every piece of apparatus used must be prescribed exactly, thus standardizing the methods of testing, if it is desired to adopt a uniform specification.

This, however, would not be the only advantage gained by the introduction of uniformity in methods. Even if there were no thought of adopting a standard specification, a standard method of testing would still be decidedly beneficial, in that it would place all results obtained on any material, in any place, on exactly the same basis. Under the present system, the results of tests in one laboratory are of little practical value to another. If, however, these results all had the same basis, they would have a direct bearing on, and be strictly comparable with the results obtained elsewhere, which would vastly increase our knowledge of the behavior of the material, and also render unnecessary a great amount of duplication of investigations.

For instance, a laboratory might make an apparently exhaustive series of tests investigating some property of cement. These tests might be repeated by a second laboratory, using different methods and probably obtaining results seemingly contradictory, thus leaving every one confused as to the indications of the tests, whereas if standard methods had been used, the results would be much more likely to have been corroborative.

Standardization of methods, therefore, would have the great advantage of placing on a common basis all results, either of routine work, or of experimental investigations.

Another benefit also would be the doing away with the constant source of friction between manufacturer and consumer in regard to the results of tests. The manufacturer's laboratory may use a method giving high results, and the consumer's laboratory may yield lower values; both, however, being accurate as regards their respective method. The manufacturer, not always realizing that the consumer's specifications are based on the results of his own laboratory, and not of the manufac-

turer's, is constantly endeavoring to show that the failure to meet the requirements is due to the consumer's methods. This is a comparatively unimportant matter, but it helps to show the many annoyances that could be obviated by standardization.

The American Society of Civil Engineers, through the report of its Committee in 1885, was among the first to inaugurate a set of rules for the Uniform Testing of Cement. While these rules served their purpose in an excellent manner for a number of years, they fail to entirely meet the requirements of to-day. This has resulted from several causes, among which may be mentioned—first, improved methods of manufacture; second, increase in knowledge of testing and better acquaintance with the properties of cement; third, the demand for greater accuracy and the more rigorous requirements in specifications; fourth, the increasing importance and magnitude of the works of construction in which cement is used as the principal material.

In recognition of these facts, the Society has recently appointed a Committee to report methods for the Uniform Tests of Cement. In carrying out these instructions the Committee is confronted with the difficulties attending the selection of standaed methods by which uniform and comparable results may be obtained. Any one engaged in the testing of cements must necessarily encounter the same difficulties and must realize that the basis of a standard specification is a system of testing which will give uniformity in results. This fact cannot be too strongly emphasized. It would therefore appear logical that the first efforts should be directed towards securing uniformity in methods of testing; then the formulation of a uniform specification would naturally follow.

## DISCUSSION.*

W. A. AIKEN.—During the past two years I have had occasion to go very intimately into the matter of testing cement in connection with my official duties as General Inspector of Material with the Board of Rapid Transit Railroad Commissioners City of New York. Long before that I had a great deal to do with cement in field work, making such tests as are possible under such conditions, but the organization necessary for the thorough inspection of a large quantity of cement, such as was called for on our New York contract, gave me the opportunity I had long desired to try to put into practice certain theories which are in agreement with what has been said by the preceding speakers, that it is absolutely necessary in a thorough system of testing to observe strict uniformity in the methods at all times. With this idea, I organized a system of testing on our Rapid Transit work, having in view the avoidance *in toto* of what is everybody's bugbear in testing cement—the "personal equation." I think I have succeeded in this to a great extent by having each set of briquettes, neat or mortar, for each lot of cement made by one man; the tests made by another man, and the records kept and reports made by a third man, each man doing the same kind of work all the time. Mr. Aiken.

As to sampling, our practice is to take a series of samples as the cement is ground into the bins, at approximately equal intervals of time or depth. Ten samples are taken from ten layers and made into neat and mortar briquettes. The former are tested after 24 hours, 7 days and 28 days; the latter after 7 days and 28 days. Besides a series of briquettes is made from a mixture of all the samples, which must give approximately the same results as the individual samples. These results are checked afterwards on briquettes made from an average of the bin as the cement is bagged and loaded into cars. I am satisfied that in this way we get representative values of all the cement sampled at the works of the American Cement Company.

* Joint discussion of the two preceding papers, viz: "Standard Cement Specifications," by R. W. Lesley, and "The Advantages of Uniformity in Specifications for Cement, and Methods of Testing," by George S. Webster.

Mr. Aiken

The agreement of results may be judged from the following data: From each bin of about 1 400 barrels, 30 briquettes are made (three from each of the ten layer samples of the bin) for each period of 24 hours, 7 days and 28 days for the neat, and 7 days and 28 days for the mortar, as well as three for each period from a mixture of all the samples. For 594 briquettes made from the sample average or mixture, representing about 230 000 barrels, the average result at the age of 24 hours was 291 lbs., whereas the average of 6 030 layer-sample briquettes was 302—a difference of 11 lbs. For 630 briquettes of the mixture at 7 days, the average result was 640 lbs., while the average for a corresponding number of briquettes at the same period from the layer samples was 672 lbs. At 28 days the mixture gave an average of 764 lbs., the layer samples 794 lbs. The percentage of variation between the gain from 7 to 28 days—a very important element in our work—as shown by the average of the layer samples and of the mixture was less than 1%.

In connection with the mortar tests, an equally if not more important matter, the variations were much smaller, the mixture giving at 7 days 386 lbs., while the average for 6 000 layer-sample briquettes at the same period was 391 lbs.—a difference of only 5 lbs. Equally good results were obtained at later periods. Thus the percentage of gain from 7 days to 6 months did not differ more than about 1% between the mixture and the average of the layer samples. I feel absolutely sure, therefore, that we obtained a representative average of all the materials and the results obtained emphasize the importance of uniformity of method in testing cement.

Mr. Taylor

W. P. TAYLOR.—In regard to uniformity of results, I may say that we tested almost 25 000 briquettes last year in the laboratory of the city of Philadelphia, and we found the probable error of each briquette to be about 7% to 8% of the mean value. I think that with the ordinary methods of manipulation it is almost impossible to secure a greater degree of accuracy than that. Our ordinary test is based on the mean value obtained from two briquettes, and their individual variation from the mean seldom exceeds 10%. There has recently been considerable discussion as to whether the highest value obtained from a series of briquettes or the mean value should be taken. I believe that

the mean value should always be taken, for the reason that the value wanted is not the greatest strength that a cement can attain; but the strength that it can attain under the conditions under which it is tested. The Army specifications stipulate that the highest value obtained from briquettes of the same molding shall determine the strength of the cement. What we want, however, is not the greatest strength that can be obtained, but the average strength under specified conditions. Mr. Taylor.

In the Philadelphia Laboratory we have done our utmost to standardize each test. We have the briquettes made regularly by one operator; broken by another; the setting tests made by a third; the analyses by a fourth; the collecting by a fifth, etc. We not only have one man trained for each test, but we have men trained to fill other men's places if anything should happen. Each operator is expected to obtain results that are practically uniform.

ROBERT JOB.—In view of the number of specifications that have been cited to-night, I should like to ask, what is regarded as the most satisfactory test for soundness? Mr. Job.

MR. LESLEY.—The answer to that would be, that in Germany, where for over ten years a commission has been making experiments on accelerated tests, the results show that the samples which have endured the ordinary cold water and air tests have endured all the tests of time. These experiments which have been going on for over ten years as stated, covering all the hot tests described in my paper, conclusively show that the cements which did not stand these hot tests, did stand the tests in work, in salt water, in air and under every other condition, indicating as to the value of the hot water, boiling and other accelerated tests simply that they were accelerated tests, and that the test pieces did boil. In other words, if a man were going to plug a boiler to be used under steam pressure, it would be very important to have a cement that boiled to plug it with; but outside of that to determine the soundness of a cement for the general uses to which cement is commonly applied, the general opinion was that the accelerated tests did not show anything, and amounted to practically nothing. Mr Lesley.

R. L. HUMPHREY.—Concerning the matter of standard specifications, I desire to state that, in my opinion, the difficulty Mr.Humphrey.

Mr. Humphrey. lies, not in the establishment of individual specifications, based on the uniform methods in vogue in the particular laboratory for which they are made, but in the correlation of these individual specifications of which Mr. Lesley has spoken. The difficulty lies in evolving a specification from a mass of individual specifications that will be standard and adaptable as a substitute for each individual specification.

The reason for the wide differences in the requirements of specifications is that each laboratory uses different methods for making the various tests. These methods, while they do not always differ radically from each other, differ sufficiently to produce a wide range in the results obtained.

As an example may be cited the 1885 rules of the American Society of Civil Engineers, which requires the cement to be molded into a "stiff plastic paste" without giving a definition of what constitutes a "stiff plastic paste" or proposing a method for obtaining such a paste. It is not possible for any two persons, without previous consultation, to mold a cement into a "stiff plastic paste" with identical or even approximately uniform results. Yet this plastic paste or consistency vitally affects the results of the tests for strength—tests on whose results is almost universally based the acceptance or rejection of a cement.

What applies to consistency applies to a greater or less extent to every other detail in connection with the methods for testing. It becomes, therefore, very important to secure a system under which uniform tests can be obtained; by such a system it should be possible for a person residing in San Francisco, after reading a description of the methods to be used, to have as correct an idea of what is required and to secure as uniform results as a person residing in the East, and their results should be comparable. This is the difficulty which confronts every one who tests cement at the present time.

I believe unquestionably that a standard specification is highly desirable, but I believe that such a specification will logically follow the adoption of uniform methods for making the tests.

Attention is directed to the fact that, in the instances cited by Mr. Lesley of the standard specifications which have been

adopted by different bodies of engineers, the specifications have been accompanied by a description of the methods to be used. It is the question of securing uniform methods for making the tests, on which we should concentrate our best thoughts and which is the first and most important step towards securing a standard specification. Mr. Humphrey

While the "boiling test" has been severely condemned, and justly so, at the same time we should not overlook the fact that it fills a very important need as a test for soundness. It is immaterial what strength a cement develops—if it does not maintain that strength, it is of no value.

The test which obtains the greatest endorsement as a test for soundness is the "cold water test." Evidences of unsoundness may develop in this test at the end of several years, or if the cement be of exceedingly bad quality, at the end of several months. But even at this shorter period, the time is too long to be of service to the American engineer.

The "boiling water," "hot water" or "steam" test seems to be the best quick test that is available. This test has unquestionably been abused, as Mr. Lesley has pointed out, and it is also true that it is not a safe test in the hands of the inexperienced, inasmuch as false conclusions are liable to be drawn, whereas failure may be due solely to faulty methods or manipulations. In the hands of the expert—and only experts should test cement—it often furnishes information of great value, and while the failure to pass the test should not necessarily condemn a cement, at the same time it should be sufficient cause to hold the cement to await the results of further examination. On the other hand, the fact that the cement has passed the test should not be taken as an indication that the cement is necessarily good.

Of all the tests to which cement is subjected, no single test can be taken as infallible, and the engineer guided by a broad experience, should determine, after a consideration of the results of all the tests, whether the cement does or does not fulfill the requirements of his specification.

There is great need of a quick test for soundness or constancy of volume. I am not prepared to say that a "hot water" or "boiling test" should be unqualifiedly adopted, but

Mr. Humphrey. I do think that some form of accelerated test should be adopted. A test for soundness or constancy of volume, a test by which it is possible to determine whether there are elements in a cement which will eventually cause it to lose its strength and durability, is by far the most important that can be applied, and one that cannot be too fully discussed. It is to be hoped that some form of accelerated test for soundness will be devised which will be entirely satisfactory.

# THE CHEMICAL ANALYSIS OF PORTLAND CEMENT: ITS POSSIBILITIES AND ITS LIMITATIONS.

By Richard K. Meade.

The value of chemical analysis of Portland Cement is becoming more and more appreciated each year. This is due to the fact that the chemical nature of cement is being better understood. Certain compounds are recognized as possessed of certain good or bad influences. In the days when cement mixture was made upon the result of the trial kiln it was natural that little faith should be put in chemical analysis, while now that well equipped chemical laboratories are as much a part of the Portland Cement plant as the kilns or the grinding machinery, more importance is being put in chemical analysis.

It is still a very common error to suppose that because chemical analysis is not an absolute guide as to the fitness of cement that it is no guide at all. It is true that the results are in themselves alone no certain indication of the quality of cement, yet coupled with the usual physical tests, they are invaluable, not only as a check upon the physical report, but also as a line on how we may expect the cement to behave on use.

Indeed, certain engineers are now giving great prominence in their specifications to the results of chemical analysis. When important engineering work is to be done, no method should be scorned which will give us any data upon which to base our opinion on the fitness of the cement to be used. No expert upon water supply would recommend or condemn any, save a very decidedly good or bad water, upon the result of a sanitary analysis only, yet no such expert would pass an opinion without having one made at all. Just as the sanitary expert compares his analysis with the topography of the land and studies the sources of contamination and the causes which tend to make each item high or low, so must the cement expert balance one item against another, and form his estimate on the whole, not from a single point. Of course, this does not apply to cements

in which magnesia or sulphur trioxide exceeds the limits or in which the lime is markedly too high or too low. These can in most cases be condemned on this one count.

Cements are usually analyzed for the following constituents: Silica, Iron, Alumina, Lime, Magnesia, Sulphur Trioxide, Carbon Dioxide, and Water. These last two are ordinarily determined together and called loss on ignition. The variation in the percentage of any one of these elements is confined between narrow limits. In a table of over 100 analyses compiled by the writer, comprising some 30 or more brands of American, English and German Portland Cements known to be of high quality, the limits of variation were as follows:

| | |
|---|---|
| Silica | 20.95 to 23.48 |
| Alumina | 5.51 to 9.74 |
| Ferric Oxide | 2.28 to 4.95 |
| Lime | 58.93 to 65.59 |
| Magnesia | 0.90 to 3.18 |

To find that the composition of a cement lies within these limits does not assure us that the cement is all right, because the hydraulic properties depend also upon the thoroughness and the way in which these elements are combined. If, however, a cement mixture is incorrectly proportioned, no amount of burning can make it a first-class cement.

The Silica, Alumina, Lime, Magnesia, etc., are combined to form three classes of compounds:

1. Compounds having hydraulic properties, such as the trisilicate and dialuminate of lime.

2. Compounds modifying the setting and hardening of cement either for good or bad, such as free lime, magnesia and calcium sulphate.

3. Inert compounds, such as uncombined clay, sand, calcium carbonate, etc.

Since we know of no compounds which absolutely modify the properties of cement for the good, the ideal cement would consist only of compounds in class 1, viz, it would be all trisilicate and dialuminate of lime. The compounds in classes 2 and 3 should not aggregate more than 10% of good cement.

Chemical analysis will show us *per se* if such deleterious compounds as magnesia, calcium sulphate or sulphide are present in sufficient quantities to injure the cement.

The analysis will also show if the cement is of normal composition. Even if the magnesia, etc., are under the limits, cements with abnormal proportions of the chief constituents, unless wanted for some special purposes, are to be looked upon with disfavor. Chemical analysis will also let us calculate by the use of the "Hydraulic Index" if the lime limit has been exceeded or if the percentage of lime is far under this. The analysis can be made to show the percentage of uncombined clay, sand, and calcium carbonate, and can be made to serve as a check upon the burning, and also upon the care with which the mixing and grinding were done.

The experiments of Le Chatelier, Dr. Newberry and others have established the hydraulic properties of Portland Cement to be dependent upon the presence of certain silicates, aluminates and ferrites of lime. Newberry and Le Chatelier both give the formula $3CaO, SiO_2$ to the most important of these compounds, the tricalcium silicate. Assuming that this formula is correct it will be seen that the silica and lime in the compound are in the ratio of 1 to 2.8. The aluminate of lime has the formula $2CaO, Al_2O_3$, according to Newberry, and $3CaO, Al_2O_3$ according to Le Chatelier. The first of these formulas is more probably correct, as it agrees closely with the best practice. Taking this as the formula for the aluminate of lime, it will be seen that the alumina and lime bear the ratio of 1 to 1.1. From this, Newberry deducted the following "Hydraulic Index" or ratio between the lime, the basic element and the silica and alumina the acid ones.

Per cent. Lime = per cent. Silica $\times$ 2.8 = per cent. Alumina $\times$ 1.1.

Most well-known Portland Cements are made from a mixture of cement rock and limestone, or of clay and marl, the composition of which is practically that shown by Newberry's formula. That they do not agree exactly is due to the fact that the formula represents the maximum of lime which will combine with the silica and alumina.

This maximum is seldom attempted by any cement maker

because of the danger of an excess of free lime in the cement. In order to get the silica, alumina and lime to combine it is necessary to grind and mix the raw materials very thoroughly and burn until the lime is all combined. If the grinding is not fine enough, the mixing not intimate enough and the burning insufficient, some of the lime will remain free or uncombined and cause the cement to crack and expand in hardening.

If cement were simply the cement mixture with the carbon dioxide and water driven off, and the lime silica and alumina properly combined, the analysis of many samples would show 90 to 95% of the lime called for by Newberry's formula. Unfortunately, cement as it comes on the market is unavoidably adulterated by the ash of the fuel used in burning and by the 1 or 2% of calcium sulphate generally added to slow its setting. The ash, as it is almost entirely silica and alumina, tends to make the lime appear too low and the cement over-clayed. The addition of the gypsum, if reported as lime and sulphur trioxide, tends to make the lime too high. These two causes act against each other, but still leave a balance in favor of the acid elements silica and alumina. Unfortunately for chemical analysis, there is no good method for differentiating between the free lime and that combined to form hydraulic compounds. The pat test only fixes a limit. This limit is probably as high as 2.5%, and in certain cements even higher than this. In some work done by the writer, consisting in adding small increasing quantities of lime to cement, the free lime in which had been saturated by carbon dioxide the checking began at about 2.0% and became marked at 2.5%. With the introduction of a method for accurately quantitatively determining free lime, chemical analysis will take first place as a method of ascertaining the value of cement.

The nearer the cement comes to having the maximum percentage of lime allowed by Newberry's formula, without showing free lime or under-burning by the pat and needle tests, the stronger the cement will be. If, on the other hand, the percentage of lime is considerably below the limit, the cement contains a large percentage of inert material, *i. e.*, uncombined clay, sand, etc., because the lime was insufficient to convert all the clay to silicate and aluminate of lime. For safety's sake,

cement always contains a small excess of uncombined clay or sand. This is shown by the percentage of silica found by fusion with sodium carbonate (or evaporation of the residue insoluble in hydrochloric acid with hydrofluoric acid) differing from that found by simple solution in dilute hydrochloric acid.

By a little modification we can easily make Newberry's formula show the minimum of lime which should be present in a good cement. Using approximate, conservative figures, it takes about 40% of the weight of the clinker of fuel to burn the former. About 10% of this coal is ash, 75% of the ash is silica and 25% is Alumina. This would make the fuel ash add 3% silica and 1% alumina to that already in the clinker. The per cent. sulphur trioxide multiplied by 0.7 will give the lime added by the gypsum. Making these corrections and assuming that the maker should be able to carry in his mixture 95% of the lime called for by Newberry's formula, we have:

$$\text{Per cent. Lime} > [(\text{per cent. Silica} - 3)\ 2.8 + (\text{per cent. Alumina} - 1)\ 1.1].\ 95 + \text{per cent. Sulphur trioxide} \times 0.7$$

or

$$\text{Per cent. Lime} > (\text{per cent. Silica} - 3)\ 2.7 + (\text{per cent. Alumina} - 1) \times 1.0 + \text{per cent. Sulphur trioxide} \times 0.7$$

No really first-class cement will fall below this minimum limit.

It will be seen that while Newberry's formula fixes the limits of the percentage of lime for a cement, the percentages of silica and alumina may vary. The properties of the compounds themselves, however, regulate the proportions of each, for the trisilicate of lime gives the hardening powers to cement, while the rate of setting is supposed to be controlled by the dialuminate of lime. Portland Cement usually contains from 7 to 12% iron oxide and alumina together. As the percentage of alumina increases in a cement, other things being equal, it becomes more and more quick setting, and on the other hand, as the silica increases under similar conditions its set becomes slower, but it will ultimately develop greater hardness.

Iron oxide is supposed to have similar hydraulic properties

to alumina. Aside from this it has a decided effect upon the color of the cement. Probably if present in the ferrous state, its action is deleterious and it also points to adulteration of the cement with blast furnace slag, or to cement burned with a strongly reducing flame. In cements made by the rotary process, examined by the writer, the ferrous iron present was under 0.2%.

The chemical analysis gives us a splendid insight into the care and skill with which the cement has been manufactured. For example, suppose that a cement contains a normal percentage of alumina and yet sets very rapidly. Then the cement is underburned or the raw materials were poorly mixed. These latter causes tend to make cement quick setting. On the other hand, if the cement sets very slowly, and does not contain an excess of calcium sulphate, then it may be diagnosed as over-burned. Again, if the cement is proportioned correctly, according to the formula given, and yet shows free lime when tested by the pat test, the raw materials were probably poorly mixed, not ground fine enough, or the burning was insufficient.

Manufacturers are more prone to sin on the side of underburning than of over-burning, since over-burned clinker is a very hard substance to grind, while under-burned clinker is much easier. Probably the separate determination of carbon dioxide and of water in cement would also aid very materially in getting a line on the burning, as well as explaining many of the freaks of setting so often met with. Fresh under-burned cement will show high loss on ignition. This loss is entirely carbon dioxide. On the other hand, cement which has stood some time and has a high loss on ignition due to saturated free lime, usually shows as much water as carbon dioxide.

Another test which gives us a good indication of the burning is the difference between the silica by solution in hydrochloric acid and that by fusion with sodium carbonate, since the trisilicate and dialuminate of lime are entirely soluble in hydrochloric acid. A simpler way of applying the test is merely to note the residue left on evaporation of the insoluble matter with hydrofluoric acid. This residue in a well-made cement should not exceed 0.3%.

In valuing cement I would suggest that the chemical analysis conform to the following limits:

Per cent. Silica 20 to 23.
Per cent. Alumina 6 to 12.
Per cent. Lime $<$ per cent. Silica $\times$ 2.8 + per cent. Alumina $\times$ 1.1.
Per cent. Lime $>$ (per cent. Silica — 3) $\times$ 2.7 + (per cent. Alumina —1) $\times$ 1.0 + per cent. Sulphur trioxide $\times$ 0.7.
Per cent. Magnesia $<$ 3½ or even 3.
Per cent. Sulphur trioxide $<$ 2 even 1½.

It should be borne in mind, however, that a change in our knowledge of how to make cement may narrow at any time these limits, more especially with regard to the minimum of lime and also of the silica, which is made unnecessarily high by contamination with the fuel ash.

## DISCUSSION.

Mr. Richardson

CLIFFORD RICHARDSON.—Mr. Meade has introduced a very interesting subject, one with which I have been more or less intimately connected for a number of years. It seems to me that physical tests will be the ones upon which the engineer will depend in a very large majority of cases, now and in the future, on account of the difficulty of ordinarily going into the matter of complete analysis of the material; unless it be the study of a new brand, or some particular samples. He may determine the amount of sulphuric acid and that of magnesia; but after he has determined that in any one brand, I think he will be so well assured that there will be little change in this brand in the immediate future he will not continue to do so. An analysis of any new brand of cement is, of course, a very important thing before one should attempt to use a large amount of it. Such an analysis may show one of two things (granted that the cement is not satisfactory): First, that it is of normal composition and has not been properly burned; or, second, that it is of abnormal composition and on that account could never be made into a satisfactory article. Chemical analysis of a cement, it seems to me, points, therefore, more to the causes of its being bad than to the fact that it is good, since results may show correct composition and yet the cement be good for nothing.

If the composition of the cement is normal, as shown by chemical analysis, there may be several reasons why it is not satisfactory in volume-constancy, strength and time of setting. In the first place, the raw material may not have been sufficiently finely ground to enable the constituents to combine at the temperature to which they have been exposed in the kiln. As an illustration of this, I may cite some work I have done in the last two years in an endeavor to corroborate the work of Newberry. He, in a paper read before the New York Section of the Society of Chemical Industry, in 1897, described several silicates and aluminates of lime and other compounds which he had prepared and which he thought probably existed in Portland cement—among them a tricalcic silicate. The German authori-

ties on cement have very generally denied the existence of the latter silicate or the possibility of making it synthetically directly from its constituents. I determined to repeat Newberry's work and to show, if possible, that they could be made. After many attempts, extending over six months, I found I could not produce the tricalcic silicate in stable form. I so told Newberry. He immediately made some and sent it to me and told me how to make it. I then made it myself without difficulty, my failure at first having been due to the fact that the finest pulverization was insufficient to put the constituents in a form where combination could be brought about at any temperature available in an ordinary gas furnace. By elutriation the silica and carbonate of lime could be obtained in a state of minute subdivision, suspending the pulverized materials in water and collecting merely that portion which would not settle out, in the case of the lime, over night; and in the case of the silica, in five or ten minutes. A mixture made of such preparation combined in the proper proportion, when burned at a temperature of 1 650° C., produced tricalcic silicate which did not dust on cooling and possessed the properties described by Newberry. Similar difficulties, not to be detected by chemical analysis may be met with in the manufacture of Portland cement. One cannot by chemical analysis tell whether the manufacturer is properly grinding his raw materials or not. It is necessary to fall back on physical tests to determine this. Mr. Richardson

On the other hand, with materials of the proper degree of fineness, a proper temperature or time of exposure to this temperature in the kiln may not be satisfactory. In that case, of course, we should have a cement which would be deficient in many respects — perhaps not volume-constant. These deficiencies would not be detected by chemical analysis, but they would be by the physical tests.

It may be added that where an abnormal raw mixture is made, containing high alumina or high lime—excessively high lime—a chemical analysis would detect that matter at once; but those abnormal features of cement would be detected as quickly from an engineering point of view by the physical as by chemical tests of cement.

The methods employed in the chemical analysis of cements

Mr Richardson must, of course, be very accurate. They have not been satisfactory up to the present time, but we have endeavored recently, through a committee of the Society of Chemical Industry, to bring about improvements in them. The methods which have been suggested will not perhaps always be employed in cement works where greater rapidity involving constant errors may be used, but in the cases where Mr. Meade favors analytical work in testing cement the most careful work done by the best methods will be necessary.

Mr. Meade has spoken in his paper of certain compounds which the Messrs. Newberry have stated exist, in their opinion, in cement. Since these investigations, A. Meyer, a Bohemian engineer in Prague, has published some results which have gone a step further than those of the Newberrys or Le Chatelier. Instead of supposing that an aluminate and one or more silicates of lime exist independently as constituents of Portland cement, that is to say, that the cement consists of dicalcic aluminate and a tricalcic silicate, Mayer has shown that the reaction resulting in the formation of Portland cement probably was as follows: At first, at a comparatively low temperature the carbonic acid is driven off from the carbonate of lime. Then follows, probably, a combination of the lime with alumina. The next step is a solution and combination of silica with the aluminate, so that we have a double silicate of the formula $SiO_2$, $Al_2O_3$, XCaO. this rather fusible slag continues to dissolve silica, comparatively rapidly, until all present is combined, and the lime as long as the temperature it meets in the furnace is sufficiently high to permit combination of lime and silica and until the amount of the former reaches five molecules. If the temperature is such that the lime will not combine with silica to that extent some remains free or in lightly bound forms and the cement is quick-setting or unstable. With five molecules of lime the material contains the proportions of silica and lime necessary to form and allow the crystallization of a tricalcic silicate and leave a portion uncrystallized. Lately in my laboratory all the different double silicates from $SiO_2$ $Al_2$ O CaO up to 5 CaO have been made synthetically; and, with five molecules of lime burned at the temperature of about 1 700° F., where the lime reaches a figure in the neighborhood of 70%, far higher than we would

find in the natural Portland cement, owing to the fact that there is no magnesia or sulphate or alkalis present; on cooling and making a thin section of it, it is found that the resulting double silicate has crystallized largely in the form of tricalcic silicate as in a piece of clinker, while the remainder is also crystalline, but in a form with quite different optical properties. Portland cement clinker is, in fact, exactly of the same structure as an igneous rock in which on gradual cooling the different elements have crystallized out in the form of various minerals, while the magma in which they are formed is left surrounding them. It is also interesting to find that if lime is reduced to four molecules the product has quite a different character, and the same may be said of that made with three, two and one molecule. All these compounds are being studied chemically and optically, and the results will shortly appear in print. Mr. Richardson

In conclusion, I should say that chemical analysis, while of extreme importance to the manufacturer in burning his cement and to the engineer in taking up any one new brand with which he has had no previous acquaintance, is not of so great importance to the ordinary individual, using Portland cement as a material of construction, as the physical tests.

# CEMENT TESTING IN MUNICIPAL LABORATORIES.

By Richard L. Humphrey.

The study of the equipment and methods in use in the various municipal laboratories, particularly as far as they apply to the tests of cement, is exceedingly interesting, and one which presents a wide field for discussion.

The first record of any systematic experiments with Portland cement were made in France by the Engineers of the Ponts et Chaussées, by whom this material was employed on a large scale as early as 1850.

Their standard of excellence provided that a Portland cement should have a specific weight of 1200 kilogrammes per cubic meter (103 lbs. per imperial bushel) and that briquettes of 1½ inch section, equal to an area of 2¼ inches, should develop the following tensile strengths:

| | Neat Cement. | 1 Cement, 2 Sand. |
|---|---|---|
| 2 days ..... | 140 lbs. | ..... |
| 5 days ..... | 280 lbs. | 140 lbs. |
| 30 days ..... | 530 lbs. | 280 lbs. |

In the latter part of 1858 John Grant began a series of tests in connection with the London Main Drainage Works and in 1859 we find the following clause in the specifications for this work:

"The whole of the cement to be used in these works is to be Portland cement of the very best quality, ground extremely fine, weighing not less than 110 lbs. per striked imperial bushel and capable of maintaining a breaking weight of 400 lbs. on an area 1½ x 1½ in., equal to 2¼ inches, 7 days after being made, in an iron mold, and immersed in water 6 of these days."

Grant maintained no central laboratory but established small laboratories on various parts of the work and equipped them with a few briquette molds, a machine for making tests of tensile strength, and apparatus for ascertaining the weight per bushel. It is interesting to note that Grant subsequently changed the form of the briquette and reduced the section to

one inch. He later added a test for fineness. Faija devised a test for constancy of volume, Vicat had an apparatus for measuring the rate of setting, while Michaelis and others added improvements in both methods and apparatus.

The methods of testing and the specifications proposed by Grant furnished the precedent which was followed by engineers generally in this country and in Europe.

In the half century which has almost elapsed since these tests were proposed, little or no change has been made in the general character of the tests, although the methods and apparatus have been considerably improved, while the quality of the cement produced, particularly of Portland cement, has been wonderfully improved.

It seems remarkable that this improvement in the quality should take place without a corresponding improvement in the methods of tests. For as Portland cement came into more general use, we find the engineer, in his anxiety to establish the qualities of this new product, applying a greater variety of tests of increasing severity. The principal qualities sought, viz, fineness, rate of setting, strength and soundness, are, however, the same to-day as in the days of John Grant. As a rule the early laboratories were field laboratories, that is they were located on the work in which the cement was to be used and the equipment was necessarily limited. Gradually laboratories of a permanent character were established for the purpose of doing more accurate work and for making investigations.

There are but few thoroughly equipped municipal laboratories in either this country or abroad. The laboratories connected with the technical schools are for the most part fairly well equipped. This is particularly the case abroad.

One of the most important of this kind is the laboratory of the Ecole des Ponts et Chaussées. This laboratory was established in 1851 as a chemical laboratory for the examination of limestones, lime, cement, mortars, pozzuolanas, and other substances employed in construction. It is interesting to note that, in the circular letter issued about this date, "the engineers of the Ponts et Chaussées and persons who desired tests and analyses made can send their samples to the laboratory which will make the examination gratuitously." This laboratory

was established as a permanent laboratory in 1884 for the systematic inspection of lime, cement, mortars, etc., used in construction of public works under the direction and charge of the engineers of the Ponts et Chaussées.

The laboratories connected with the technical schools at Munich, Charlottenburg, the laboratories at Zurich and St. Petersburg, are exceedingly well equipped and are making many valuable and important investigations.

The laboratory at Charlottenburg is a particularly elaborate affair, being provided with separate rooms for individual investigators.

In 1877 the Association of German Cement Manufacturers was formed. This association has by its efforts accomplished more towards the unification of methods of testing and the improvement of the quality of cement than any other society, and its standard rules adopted throughout Germany have been closely followed by the engineering profession generally.

In this country among the very earliest municipal laboratories was that established under the Engineer Commissioner of the District of Columbia by the first Board of Commissioners in 1878 and under the charge of Mr. E. J. DeSmedt. Its original purpose was for the use of Mr. DeSmedt in laying the first asphalt pavement in Washington. Mr. DeSmedt was succeeded by Mr. Clifford Richardson in 1887 and the latter by Mr. A. W. Dow in 1894.

The Department of Docks of the city of New York established a laboratory for testing the cement used in the construction of its docks in 1870. Three years later Captain W. W. Maclay assumed charge of the laboratory. Many of the earlier contributions to cement literature were the result of the investigations made in this laboratory. Captain Maclay also devised the Maclay hot bath tests which were so generally adopted in this country. The Boston Main Drainage Board in 1878 established a laboratory under the direction of Mr. Elliott C. Clark. The equipment resembled that of a field laboratory.

Under the Metropolitan Sewerage Commissioners of Massachusetts an elaborate and well equipped laboratory was established at their principal office in Boston in 1890 at considerable expense under the charge of Mr. Nelson A. Hallett. This labora-

tory was unfortunately destroyed by fire in 1893 and since this time the Commissioners have expended no large sums on permanent laboratories.

They have, however, equipped several field laboratories at convenient points along the work.

In the *Engineering News*, January 3, 1891, is an article by Mr. S. Bent Russell describing the laboratory of the St. Louis Water Works Extension, established about 1888 and for which he was for a number of years in charge. In 1895 the laboratory was removed to a point further down town and the Sewer and Street Departments joined with the Water Department in operating it for testing cement, paving brick and other materials.

The city of Baltimore's laboratory for testing cement was established about 1891 under the direction of Mr. C. B. Marriott. The equipment of this laboratory was meagre and the methods crude and it has been succeeded by the laboratory established in 1900 under the direction of the Highway Division of the State Geological Survey. The latter is equipped principally with apparatus for testing the materials used in road construction; the equipment for cement tests being of a rather limited character.

The laboratory of the city of Chicago was established in 1894, with Mr. S. M. Rowe in charge. The equipment of the laboratory is fairly complete and consists of a 1000 lb. Fairbanks cement testing machine, a 20 000 lb. Riehlé tension and compression machine, storage tanks with a capacity for 7 000 briquettes, together with the necessary molds, set wires, scales, etc.

New York has from time to time maintained laboratories for testing cement under the direction of the various departments. The equipment of these laboratories was limited and more or less crude; was adapted for field work only, and therefore met with indifferent success.

In 1899 a laboratory was fitted up by the Commissioners of Accounts for the purpose of testing all the material used by the city of New York. The laboratory is under the direction of Mr. Otto H. Klein, Chief Engineer, with Professor S. F. Peckham, Chemist in charge. The equipment, while on a small scale, owing to the limited space set aside for the purpose, is good and sufficient for making most of the usual tests.

Prior to 1892 the inspection and testing of cement in the city of Philadelphia was done in field laboratories and usually on important work only. In that year there was started, in an obscure corner of the City Hall, in a very modest way a permanent laboratory for the testing of cements. The equipment consisted of a Fairbanks testing machine, a set of Gilmore wires, an 8 gang briquette mold and a few pans for preserving the briquettes. The work was necessarily of a limited character, although it was performed in a systematic manner and the results were carefully compiled. Gradually additional apparatus was added, better facilities provided and the scope of the work enlarged. The result of this rather unpromising beginning is a permanent laboratory, with facilities and equipment for making all the tests in general use, whose equal does not exist in this country and which is excelled by but few laboratories abroad.

The equipment and methods in use in this laboratory are described in part in a paper before the Engineers' Club of Philadelphia, and I will not take the time to dwell at greater length on these features. As the Engineer in charge I had the pleasure of supervising its development from its inception.

This laboratory has done a very great deal towards raising the uniformity and standard of quality of the American cements, and its published tests were among the first to show the superiority of the American over the foreign cements. The improvements in the quality of the cements furnished the city and the reduction in the cost, incident to the competition which the methods in vogue in the laboratory made possible, have more than compensated the city for its cost of installation and maintenance. The cost of maintenance does not exceed 10% of the probable annual saving to the city.

This covers in a brief general way the history of the principal municipal laboratories in this country.

Laboratories are also maintained by the cities of Cleveland, Indianapolis, Buffalo, New Orleans and others, but they are for the most part on a smaller scale and possess no unusual features.

The number of municipalities which maintain laboratories of this character is constantly increasing, although it is to be regretted that neither in facilities, equipment nor methods do they attain the necessary and desired standard of efficiency.

Municipal laboratories may be divided into two general classes: (1) Field laboratories, or those located on the work in which the cement is to be used. Such laboratories are equipped for making only a few simple tests and the work is not usually carried on in a systematic manner; and (2) permanent laboratories, well equipped with apparatus for the thorough investigation of the properties of the materials of construction. Work in the laboratories of this class is carried on in a thoroughly systematic way and the results are carefully compiled and made available for general use.

Aside from private laboratories, more or less well equipped, which do commercial testing, there is a class connected with the technical schools. The latter are generally well equipped and many of them make a practice of doing commercial testing and in a few cases have charge of the inspection of the materials used by neighboring cities and towns, while a few others extend the use of their laboratories to the city engineers and county commissioners of the state in which they are located. Lehigh University is an example of this class.

The work of testing in these laboratories is usually entrusted to the instructors and in many cases the students themselves. As there is generally a lack of the requisite experience and skill, the results obtained, under these conditions, are of doubtful value.

While the system under which all cements are tested far from fills the requirements (owing largely to the great influence of the personal equation of the persons who make the tests) it is nevertheless true that the tests and inspection to which cements are subjected has done more towards improving the quality of cement than any other agency.

The manufacturer, in order that his product may fulfill the requirements, has exerted greater care in the preparation of his product and with a corresponding increase in its quality both as regards its strength and uniformity.

The advent of the rotary kiln has proved another potent factor in the improvement of the quality of Portland cement; indeed the present high standard of quality is due largely to this process.

Without systematic inspection on the part of the consumer,

the manufacturer having no requirements to meet would fall into careless habits and the standard of quality of his product would gradually fall. When John Grant first proposed a tensile strength of 400 lbs. on 1½ in. section at end of 7 days the English manufacturers exerted considerable pressure to have the requirement reduced to 300 lbs., fearing they would experience great difficulty in meeting it. That this fear was groundless is shown by the fact that within a very short time afterward the requirement was raised to 550 lbs.

From the compiled results of tests we learn what qualities cement may be reasonably expected to develop. We further learn that those cements which yield the best results in actual use, readily meet certain specified requirements. It is therefore natural for the engineer to fix a standard of excellence (based on the results which he has obtained) by which to gage the acceptability of each shipment. So long as the engineer fixes the requirements of his specifications in this manner, there is small likelihood of a serious disagreement between the engineer and the manufacturer, as to whether the cement does or does not meet the requirements.

It is the class of consumers, however, who draft their specifications with the aid of a pair of shears, that occasion the greatest trouble, not only to themselves but also to the manufacturers. Such persons as a rule with their limited knowledge of the properties of cement, select for their specifications the most rigid clauses from a number of other specifications, and the result is a new and impracticable standard, calling for impossible and often contradictory conditions.

The manufacturer, unless he has a previous knowledge of the personal equation of the person in charge of the laboratory, is loth, therefore, to submit his product and chance the possible rejection of a good material required to meet these impossible conditions. It is not surprising that under such conditions the manufacturer refuses to supply cement. As a result the consumer is placed in the embarrassing position of being obliged either to disregard his specifications or change the requirements.

A uniform or standard specifications is *unquestionably* very desirable, but the realization of such a standard is only possible through the adoption of such methods of testing as will secure

uniform and comparable results by different persons. A Board of Engineers have prepared a set of specifications for the guidance of the Corps of Engineers of the United States army, but it should be noted that they have prefaced these specifications with a detailed description of the methods to be used. It would seem therefore that the first step towards securing uniform specifications is the adoption of methods for making uniform and comparable tests. When this has been successfully accomplished standard specifications will not only be possible but will also be the natural sequence.

The testing of cement is an art requiring a combination of skill and experience, and a natural inclination towards scientific research on the part of the person making the tests. The wretchedly poor salaries generally paid those in charge and the correspondingly incompetent assistants which such salaries secure, is one of the deplorable features of the municipal laboratories of to-day. An exception may perhaps be made of a few of the larger laboratories where the assistant in charge is content to accept the poor pay for the sake of the reputation and experience to be acquired. As a rule the assistants in charge have little interest in their work and frequently resign their positions after a few years' service just when they have become experienced and skillful and therefore valuable. The work accomplished under such conditions is inefficient and unsatisfactory. As a result the great bulk of the tests turned out by municipal laboratories, through lack of uniformity in methods and skill on the part of those making the tests, are of little value. It is little wonder, therefore, that there should be such wide difference in the requirements of the specifications of these laboratories. It will be found that the specifications issued by laboratories properly equipped and in charge of skilled persons agree much more closely. Municipalities can well afford to maintain thoroughly equipped testing laboratories, conducted systematically and in charge of technically trained engineers, whose compensation will be sufficient to retain their services for a number of years after they have acquired the requisite skill and experience which make them competent and valuable. The wisdom of such an expenditure of time and money will become apparent when the improved quality of the materials (furnished at a reduction in

cost) and the increased durability of the work constructed with these materials, are taken into consideration.

The number of regular tests to be made in such a laboratory should be few and the methods used the most simple. Ordinarily the tests for determination of strength neat and with standard sand for seven and twenty-eight days, the degree of fineness, the rate of setting and some simple efficient test for constancy of volume should be sufficient. If the laboratory possess the services of a skilled chemist, chemical analyses should also be made.

For further information concerning methods and apparatus to be used in making such tests those interested are referred to the *Journal of the Franklin Institute* for December, 1901, and for January and February, 1902.

Another condition, resulting largely from the maintenance of municipal laboratories, particularly those conducted on broad lines, is the better *esprit de corps* which now exists between the engineer and the manufacturer. The old situation where these parties held aloof and viewed each other with suspicion and often contempt was an intolerable one, and I am glad to say is rapidly disappearing. The advent of the technically trained assistant at the factory and in the laboratory of the consumer, has done much to bring about this condition. Instead of working at cross purposes, both parties are bringing their combined knowledge and skill to bear on the perplexing and difficult problems which confront the producer and consumer alike and are coming more closely together in an endeavor to effect a solution.

At the present time only a few of the better equipped municipal laboratories compile and publish in a systematic manner the results of their tests. These tests serve as a guide to aid the engineer, the architect or the builder (who possess no facilities for making tests of this character), in selecting the best and most suitable material for their work. As these results are obtained under different conditions, they are not related and there seems to be no basis for a comparison. Without a knowledge of the methods in vogue and the personal equation of the engineer in charge, it is impossible to form any idea of the relative quality of two cements, where one is tested in one laboratory and the other in another. There is therefore a most urgent need

for a central or reference laboratory, conducted under impartial conditions, at the service of the general public, where fixed standards can be maintained and to which all materials of construction can be referred in case of dispute between the producer and consumer or for purposes of general information as to its properties.

Several foreign laboratories, notably that at Charlottenburg, Germany, are conducted on these lines, under government direction and are of immeasurable value. There is no government laboratory of this character in this country at the present time. Most manufacturers and many engineers will no doubt agree that there is a growing need for the same. Such a laboratory could establish standards of excellence and could conduct its tests in such a manner that the published results would be related and be of general use. The compensation of the assistants in charge should be sufficient to secure and retain the services of competent persons of great skill and broad experience.

Neither municipal nor private laboratories have the requisite time, as a rule, for carrying on research work of any magnitude. A government laboratory aside from its use as a standard reference laboratory could very properly conduct investigations with a view of solving some of the very perplexing problems connected with the production and use of cement.

The object of these remarks is to show the importance of well equipped laboratories in charge of skilled persons, to municipalities, to point out possible economies both as regards the material and the durability of the work resulting from their maintenance, and to deplore the inadequate salaries paid, which prohibits securing and retaining properly trained assistants.

The shortsightedness of the policy of employing properly paid skilled engineers to design public works, and then to entrust the inspection of the material on which their strength and durability depends to unskilled assistants is, I think, evident.

Another object is to call attention to the growing need of a government laboratory equipped with the latest and best appliances, using the most reliable methods and under the charge of the most competent experts available, to serve as an arbitrator for all disputes and as a standard by which to gage the quality of all materials of construction.

We are now entering on a new era, an era which has been appropriately called the "age of cement."

The varied applications of cement in constructive work, especially the rapidly increasing use of "steel concrete," renders a better knowledge of the strength and properties of the principal material used highly essential. The accurate determination of the properties of this material are more difficult than for any other material of construction. It is of vital importance that this material should not only harden rapidly and attain great strength, but what is far more essential, that it shall maintain this strength.

A knowledge of the quality and properties of cement becomes therefore a matter of the greatest importance and demands that the testing should be conducted in laboratories possessing the best possible equipment, using the best methods available and under the supervision of skilled technically trained assistants.

# TESTS OF REINFORCED CONCRETE BEAMS.*

By W. Kendrick Hatt.

*General Statement.*—The writer has elsewhere† presented a theory of the strength of reinforced concrete beams. Since that time he has had an opportunity to carry out a series of tests on such beams in the Laboratory for Testing Materials of Purdue University, with a view to determining the reliability of the theory. The present paper describes the methods and records the results of the tests. Incidentally the theory advanced in the article cited has been improved and the results of its application compared with the observed facts of the tests.

The writer desires to acknowledge the faithful services of Messrs. A. E. Kemmer, H. O. Garman, C. Klueh and M. R. Keefe, senior students in the School of Civil Engineering, who, as a thesis exercise, under the writer's supervision, molded the test specimens, tested the same and worked up the data obtained.

The scarcity of recorded data giving results indicative of the nature of reinforced concrete under flexure is the motive which prompts the presentation of the results of the series of tests described below. The theoretical analysis is also believed to be of value.

## Tests.

These tests include observations of the load-deflection curve for beams of broken-stone concrete reinforced with 1% and 2% of wrought iron‡ located at different positions in the cross-section of the beam. A few tests on gravel and cinder concrete beams are added. The beams were 8 ins. x 8 ins. in cross-section, and were tested on a span of 80 ins. under a centre load. Beams Nos. 1 to 17 were 1—2—4 stone concrete; beams Nos. 18, 19 and 20 were of 1—2—4 cinder concrete; beam No. 21 was of 1—5 gravel concrete. The reinforcement was placed either 1 in.

* Acknowledgment is made to the Engineering News Publishing Company for the use of the cuts used in this paper.

† Proceedings Indiana Engineering Society, January, 1902; *Engineering News*, February 27, 1902

‡ More exactly $\frac{221}{640}$ % and $\frac{1292}{640}$ %.

or 2 ins. from the bottom. Accompanying the tests in flexure is a determination of the following elements of the strength of the constituents: Moduli of elasticity and strength, in tension and compression, of the broken-stone concrete; the strength of the cement in tension and compression; the elastic limit and modulus of elasticity of the iron; analysis of the sand and stone; tests of the adhesion between the iron and the concrete.

*Cement.*—The cement used was Peninsular brand of Portland cement, made from marl and clay at Jackson, Mich. The strength of the cement in lbs. per sq. in. is shown by Table I:

TABLE I. (SHOWING TENSILE AND COMPRESSIVE STRENGTH OF PORTLAND CEMENT USED IN MAKING REINFORCED CONCRETE BEAMS.)

Tensile Strength of Standard Briquettes.

| Age | 24 hrs. | 7 days. | 1 month. | 3 mos. |
|---|---|---|---|---|
| Neat | 346 | 803 | ..... | 802 |
| 3 : 1 | 78 | 412 | 505 | 626 |

Compressive Strength of 2-in. Cubes.

| | | | | |
|---|---|---|---|---|
| Neat | 2 402 | 7 337 | 9 537 | 9 740 |
| 3 : 1 | 610 | 3 145 | 5 860 | 8 005 |

Ratio of Compressive to Tensile Strength.

| | | | | |
|---|---|---|---|---|
| Neat | 6.94 | 9.13 | ..... | 12.14 |
| 3 : 1 | 7.82 | 7.63 | 11.60 | 12.78 |

Slow setting: Fineness—98% through No. 100 sieve; boiling test—O. K.

*Broken Stone.*—The broken stone was limestone; the entire product of the crusher was used; 73% was retained on a ¼-in. sieve; all passed through a 1-in. sieve.

*Sand.*—The sand used was clean, sharp pit sand; 84% was retained on a No. 30 sieve; 20% was retained on a No. 20 sieve.

*Gravel.*—The gravel used was good quality of coarse gravel.

*Cinders.*—The cinder used was that of bituminous coal, taken directly from the cinder pile, and contained about 50% fine ash.

*Mixing, Molding and Storage.*—The cement and sand were thoroughly mixed in a dry condition, and then the water was applied; then the broken stone the surfaces of which had been sprayed with water, was added to the mortar and the whole

thoroughly mixed. The concrete was fairly dry concrete intended to be plastic after a thorough ramming. About 5.5% of water by weight was added to the mortar. Some of the specimens were more wet than others; these facts are noted in the individual cases in Table II. The specimens were stored in

TABLE II. (SHOWING CONDITION OF CONCRETE BEAMS WHEN MADE AND AFTER TESTING.)

| | Condition when Made. | After Test. | W't, lbs. |
|---|---|---|---|
| No. 1. | Concrete normal ..... | Crack 4″ from centre | 480 |
| " 2. | " " ..... | " at centre ..... | 485 |
| " 4. | " " ..... | " 2″ from centre | 490 |
| " 5. | " " ..... | " at centre ..... | .... |
| " 6. | " " ..... | " " ..... | .... |
| " 7. | " rather wet..... | " " ..... | .... |
| " 8. | " liver consistency ..... | " " ..... | .... |
| " 9. | " liver consistency ..... | " " ..... | .... |
| " 10. | " very wet ..... | " 6″ from centre | .... |
| " 11. | " rather dry ..... | " 6″ from centre | .... |
| " 12. | " liver consistency ..... | 2 cracks, 2″ each side of centre ..... | .... |
| " 13. | " normal ..... | Crack 3″ from centre | .... |
| " 14. | " rather dry ..... | Tested at half span.. | .... |
| " 15. | " normal ..... | Crack at centre.... | 480 |
| " 16. | ' rather dry ..... | Crack 4″ from centre | 500 |
| " 17. | " normal ..... | " at centre ..... | .... |
| " 18. | " normal ..... | " 3″ from centre Failed in compression later..... | .... |
| " 19. | " too dry ..... | Crack 4″ from centre | .... |
| " 20. | " normal ..... | " at centre ..... | .... |
| " 21. | " normal ..... | Tested at half span.. | .... |
| " 22. | Gravel concrete, 1 : 5..... | Failed suddenly by horizontal shear along reinforcement, end of beam. | .... |

NOTE.—Nos. 1 to 17 are broken stone concrete, 1 : 2 : 4; Nos. 18 to 21 are cinder concrete, 1 : 2 : 4; No. 22, gravel concrete, 1 : 5.

dry sand and the rate of hardening was rather more rapid than that to be expected in the case of a cement walk. The specimens were taken from the molds as soon as they could be handled without danger of breakage. This period was from 24 to 48 hours in case of the beams made in the early spring, and from 12 to 24 hours in case of beams made at a later period. The weight of the stone concrete was nearly 152 lbs. per cubic foot.

*Specimens.*—The compression tests were made on cylinders 8 ins. in diameter, and 12 ins. high. The tension tests were made on bars of square section 4 ins. on the side. The form of the tension specimens is shown in Fig. 1. These tension specimens were not well designed, since the heads pulled off before rupture occurred in the body of the plain bars. The reinforced bars, however, broke in the body. The tension tests included both plain bars and reinforced bars. Wooden molds were used for these specimens. The beams were 8 ins. square and 80 ins. between supports. The steel mold for these beams was built up of two channels and a bottom plate. The adhesion between the iron and concrete was determined by observing the load necessary to pull iron rods out of 6-in. cubes of concrete.

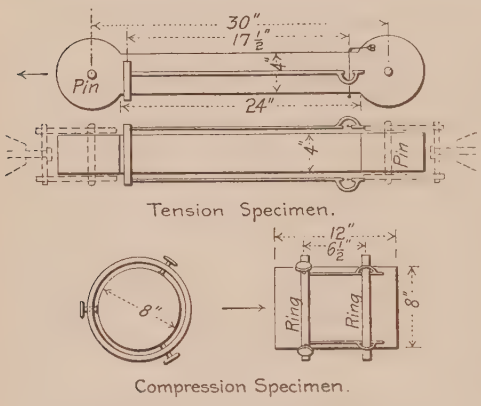



FIG. 1. Details of Tension and Compression Test Pieces.

## METHODS OF TESTING.

*Compression.*—Tests were made both on plain cylinders and on cylinders reinforced with rods parallel to the axis of the cylinders. No circumferential rods were used. The tests on these reinforced specimens are not quoted below. These reinforced cylinders were weaker than the plain cylinders, and the forms of rupture were abnormal. The shortening of all the cylinders was observed at three points around the circumference by means of hook micrometers attached to a lower steel ring (Fig. 1) and having contact with an upper steel ring, the gage length being about 6½ ins. The contact was noted by means of an electric current and the micrometers read to 1-1000-in. and estimated to 1-10 000-in. These tests were satisfactory.

*Tension Tests.*—The extensions of the tension bars were read on hook micrometers attached to an upper yoke and having contact with fine wires fixed by plaster of Paris to the specimen at the lower end of the gage length. The contact was made known by the passage of the electric current; the gage length was about 17½ ins.

These tension tests were not satisfactory for determination of strength, since, as noted above, the heads of the bars, with some exceptions, pulled off. They served, however, very well for determination of modulus of elasticity. It is believed by the writer that the strength of the body of the bars did not differ greatly from the loads recorded at the point of rupture of the heads. The strength of the reinforced bars which broke in the body was nearly the same as that of the plain bars which broke in the head, viz: about 300 lbs. per sq. in. The stress-strain diagrams obtained in case of the reinforced bars are important elements of the tests.

*Flexure.*—The beams were simply supported at the ends and loaded with a center load. The deflections were read at the middle of the span on both sides of the beam from hook micrometers attached to the beam and making electric contact with wires stretched between points directly over the supporting knife edges, and 4 ins. above the latter.

In the three kinds of tests mentioned, loads were applied by increments, and the deformations noted. The loads were released at intervals in order to obtain the set.

## RESULTS OF TESTS.

*Flexure Tests.*—The load-deflection diagrams obtained from the concrete beams are shown in Figs. 2, 3 and 4. Notes on condition of concrete and on rupture of beams are given in Table II.

*Compression and Tension Tests.*—Table III gives the results of the compression and tension tests. The modulus of elasticity is computed with regard to the set experienced after previous loads. The stress quoted in case of reinforced concrete bars in tension is not corrected for the amount of the load carried by the reinforcement.

*Tests of Adhesion.*—Table IV gives adhesion of round steel rods inserted in cubes of concrete. The values of adhesion rep-

TABLE III. (SHOWING RESULTS OF TENSION AND COMPRESSION TESTS OF CONCRETE.)

TENSION.

| No. | Kind Stone. | Age in days. | Modulus of elasicity, lbs. per sq. in. | Elongation at rupture, 1 part in | Strength, lbs. per sq. in. | Where broken. |
|---|---|---|---|---|---|---|
| 1. | 1—2—4 | 35 | 2 700 000 | 11 660 | 300 | At pin. |
| 2. | 1—2—4 | 33 | 2 400 000 | 8 750 | 305 | " |
| 3. | 1—2—4 | 28 | 1 400 000 | 4 400 | 360 | " |
| 4. | 1—2—4 | 26 | 1 900 000 | 7 700 | 280 | " |
| Average of plain ..... | | | 2 100 000 | 7 000 | 311 | ..... |
| 5. | 1—2—4 1% iron. | 28 | ..... | 910 | 281 | Body. |
| 6. | " | 26 | ..... | 1 310 | 313 | " |
| Average of reinforced ..... | | | ..... | 1 140 | 297 | ..... |
| | Plain cinder. | | | | | |
| 7. | 1—2—4 | 11 | ..... | ..... | 82 | Body. |

NOTE.—Values for reinforced specimens are not corrected for stress in iron.

COMPRESSION.

| No. | Kind Stone. | Age. | Modulus of elasticity lbs. per sq. in. | Stress of | Compressive strength |
|---|---|---|---|---|---|
| 1. | 1—2—4 | 9 | 4 702 000 at | 750 | 2 880 |
| | | 9 | 3 940 000 " | 1 500 | |
| 2. | 1—2—4 | 14 | 4 340 000 " | 750 | 2 575 |
| | | 14 | 3 680 000 " | 1 500 | |
| | Cinder. | | | | |
| 3. | 1—2—4 | 9 | 558 600 | ..... | 495 |
| 4. | 1—2—4 | 9 | 553 000 | ..... | 595 |
| 5. | 1—2—4 | 7 | 630 000 | ..... | 416 |
| | Gravel. | | | | |
| 6. | 1—5 | 6 | 2 088 000 | ..... | 1 185 |

$$n = \frac{E_o}{E_t} = 2.17 \text{ at } 750 \text{ for A.}$$

$$m = \frac{E_o}{E_t} = 12.8 \text{ at } 1\,500 \text{ for crack.}$$

} Stone concrete.

TABLE IV. (SHOWING ADHESION OF ROUND STEEL RODS INSERTED IN CUBES OF STONE CONCRETE.)

| | | |
|---|---|---|
| Diameter of rod | 7-16 in. | 5-8 in. |
| Age | 32 days. | 35 days. |
| Depth of rod in concrete | 6 ins. | 6.4 ins. |
| No. of tests | 3 | 3 |
| Adhesion, lbs. per sq. in. of surface of rod: | | |
| Maximum | 735 | 780 |
| Minimum | 470 | 714 |
| Average | 636 | 756 |

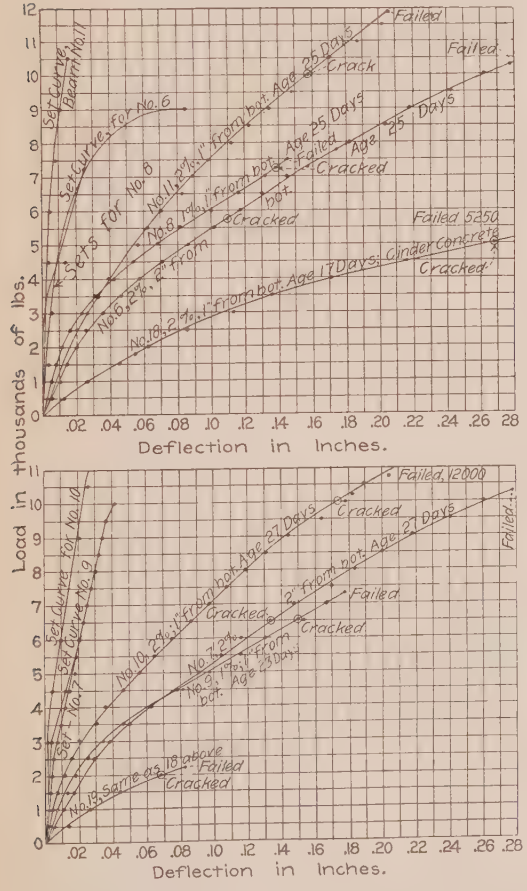



FIG. 2. Load-Deflection Diagrams of Reinforced Concrete Beams.

resent the ultimate statical resistance in pounds per square inch of the surface of the rod nominally in contact with the concrete, against a direct tension. Sliding friction, after the adhesion was overcome, was from 50% to 70% of the adhesion. In many cases the concrete cubes were broken with a hammer after the test in order to determine the uniformity of the contact between the iron and the mortar. This contact obtained only in part, and was irregularly distributed over the surface of the rods.

*Tests of Iron.*—The metal used was common wrought iron rods, 7-16 in. and 5-8 in. in diameter, respectively. The iron

had a yield point of 36 000 lbs. per sq. in., and a modulus of elasticity of 29 000 000 lbs. per sq. in.

## ANALYSIS OF RESULTS.

*The Nature of the Load-Deformation Diagram of Reinforced Concrete Beams.*—From an inspection of Figs. 2, 3 and 4 it will be seen that the diagram is nearly a straight line up to a load of from 1 500 to 3 000 lbs. in the various beams tested. At higher loads the deflection increases more rapidly. The diagram, however, again becomes nearly a straight line, the deflection then increasing uniformly with the load until at a load of from 4 000 to 10 000 lbs., a crack occurs in the concrete at the lower face of the beam. Beyond this point the deflection still increases uniformly with the load until the iron reinforcement reaches its elastic limit, at which time the deflection increases rapidly without any corresponding increase of load. These remarks refer mainly to the broken-stone beams one month old. Fig. 5 shows typical diagrams for concrete beams in flexure.

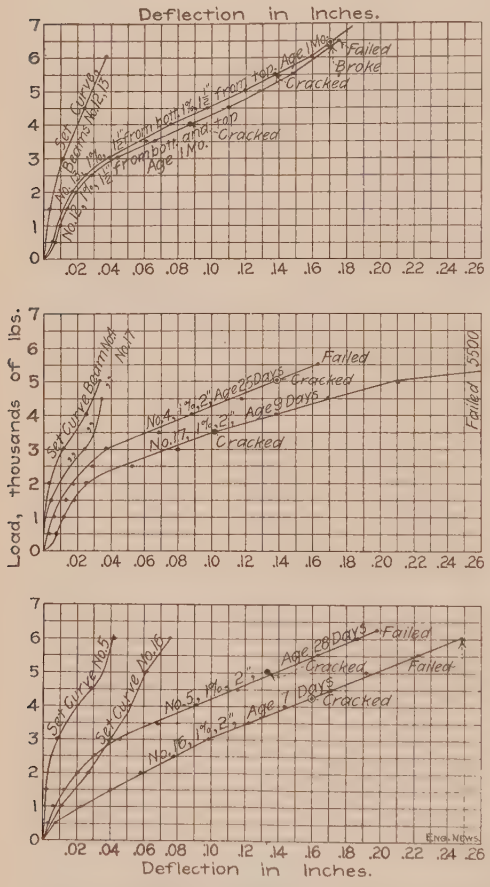



FIG. 3. Load-Deflection Diagrams of Reinforced Concrete Beams

The three characteristic points of such a test are: the some-

what indefinite point A, where the curve first turns from the straight line; the point of first crack of the concrete, and the point at which the elastic limit of the reinforcing metal is reached. Table V is a summary of the facts of the test of the series of concrete beams listed with reference to these three points. The loads in Table V are in addition to the dead weight of the beams.

It may be said that in none of the stone-concrete beams from Nos. 1 to 17, inclusive, was there any indication that the compressive strength of the concrete was reached at the load at which the reinforcement failed. If steel reinforcement had been used the compressive strength of the concrete might have been developed. Nor, as might have been expected from the results in Table IV, did the reinforcement pull out from the surrounding concrete. It should be noted that the bars were not subjected to vibrations. The cement, it may be noted, was strong enough to break the stones in the section of rupture of the beam.

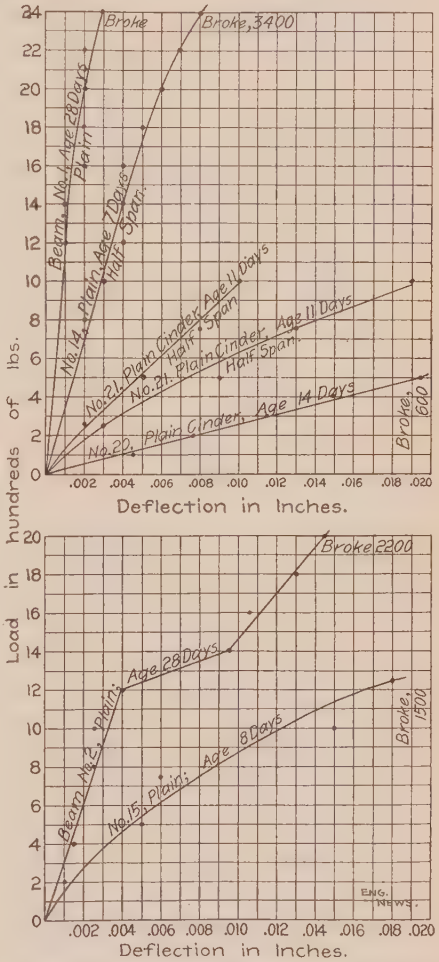

Broke
Broke, 3400
Beam, No. 1, Age 28 Days, Plain
No. 14, Plain, Age 7 Days, Half Span.
No. 21, Plain Cinder, Age 11 Days, Half Span.
No. 21, Plain Cinder, Age 11 Days, Half Span.
No. 22, Plain Cinder, Age 14 Days
Broke, 600
Load in hundreds of lbs.
Deflection in Inches.
Broke 2200
Beam No. 2, Plain; Age 28 Days
No. 15, Plain; Age 8 Days
Broke, 1500
ENG. NEWS.
Deflection in Inches.

FIG. 4. Load-Deflection Diagrams of Reinforced Concrete Beams.

*Ratio of the Moduli of Elasticity.*—From Table III it may be seen that the ratio of the modulus of elasticity of stone concrete in compression to that in tension was 2.17, corresponding to a stress of 750 lbs. per sq. in. in compression, and a stress of

300 lbs. per sq. in. in tension. It is to be noted that the ratio of the modulus of elasticity of the iron to that of the concrete in tension was 13.9. These values will apply to the beam when loaded to the point A. The important fact is the increased extensibility of the reinforced concrete in tension. Thus, while the plain concrete broke with an average extension of 1 : 7 000, the reinforced concrete broke with an average extension of 1 : 1 140. This fact has been determined by M. Considère, who says that plain concrete breaks in tension with an elongation of 1 part in 10 000, while reinforced concrete will withstand an extension of 1 part in 1 000 without rupture. The effect of reinforcement probably is to distribute the maximum elongation over the entire length of the bar, whereas, in case of plain concrete, the maximum elongation is confined to the fractured section.

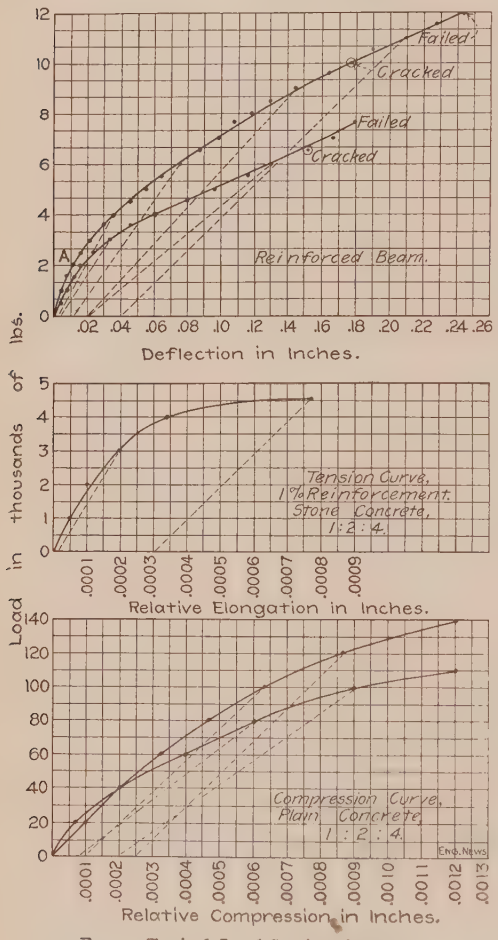



FIG. 5. Typical Load-Strain Diagrams.

Accepting the tensile strength of the reinforced concrete as 300 lbs. per sq. in., and the elongation as 1 part in 1 000 at rupture, the equivalent modulus of elasticity of the reinforced concrete in tension is 300 000 lbs. per sq. in. at the stress of 300 lbs. per sq. in., *i. e.*, at the point of cracking.

TABLE V. (SHOWING RESULTS OF BENDING TESTS OF REINFORCED CONCRETE BEAMS.)

| Beam No. | Kind. | Age in days. | Point A | | Crack | | Failure | |
|---|---|---|---|---|---|---|---|---|
| | | | Load, lbs. | Defl., in. | Load, lbs. | Defl., in. | Load, lbs. | Defl., in. |
| 1 | Plain. | 28 | 1 200 | .001 | ..... | .... | 2 400 | .003 |
| 2 | Plain. | 28 | 1 200 | .004 | ..... | .... | 2 200 | .014 |
| 4 | 1%—2″ | 25 | 2 500 | .026 | 5 000 | .140 | 5 500 | .163 |
| 5 | 1%—2″ | 28 | 2 000 | .020 | 5 000 | .136 | 6 250 | .186 |
| 6 | 2%—2″ | 25 | 2 000 | .020 | 5 750 | .109 | 10 250 | .278 |
| 7 | 2%—2″ | 27 | 2 500 | .030 | 6 500 | .133 | 10 250 | .278 |
| 8 | 1%—1″ | 25 | 2 500 | .016 | 7 250 | .138 | 7 500 | .145 |
| 9 | 1%—1″ | 23 | 2 500 | .025 | 6 500 | .150 | 7 300 | .177 |
| 10 | 2%—1″ | 27 | 3 000 | .022 | 10 000 | .174 | 12 000 | .247 |
| 11 | 2%—1″ | 25 | 3 000 | .026 | 10 000 | .158 | 11 800 | .208 |
| 12 | 1%—1½″ | 30 | 2 000 | .020 | 4 000 | .087 | 6 500 | .176 |
| 13 | 1%—1½″ | 30 | 2 000 | .017 | 5 500 | .138 | 6 400 | .170 |
| 14* | Plain. | 7 | 1 400 | .004 | ..... | .... | 3 400 | .008 |
| 15 | Plain. | 8 | 200 | .0014 | ..... | .... | 1 500 | .018 |
| 16 | 1%—2″ | 7 | ..... | .... | 4 250 | .160 | 6 000 | .248 |
| 17 | 1%—2″ | 9 | 2 000 | .026 | 3 500 | .101 | 5 500 | .280 |
| 18† | 2%—1″ | 17 | 1 000 | .026 | 5 000 | .268 | 5 250 | .288 |
| 19† | 2%—1″ | 17 | 500 | .014 | 2 000 | .068 | 2 300 | .082 |
| 20† | Plain. | 14 | ..... | .... | ..... | .... | 600 | .023 |
| 21* | Plain.† | 11 | 205 | .003 | ..... | .... | 1 100 | .019 |
| 21* | Plain.† | 11 | 205 | .002 | ..... | .... | 1 100 | .010 |
| 22‡ | 3%—1″ | 6 | 4 000 | .036 | 10 000 | .124 | 11 500 | .160 |

The new ratio of the modulus of elasticity of concrete in compression to that of reinforced concrete in tension is then 12.8 when these moduli are taken at stresses of 1 500 and 300 lbs. per sq. in. in compression and tension, respectively. The corresponding ratio for the iron and concrete is 96. These values will apply to the beams when loaded to the point of first crack. The bearing and importance of these ratios in the computation of the strength of such beams will be shown below.

Fig. 5 shows typical load-deformation curves also for concrete in compression and reinforced concrete in tension. The loads are total loads on cross-sectional area of specimens. From an examination of a number of stress-strain diagrams for concrete in compression, it appears that a parabolic arc closely approximates the actual curve.

*Analysis of the Effect of Variables.*—In the beams tested, the

* ½ span. † Cinder. ‡ Gravel. In Nos. 12 and 13 there was a double reinforcement of 1% placed 1½ inches from top and bottom face. All other beams were singly reinforced

variables were age, per cent. of reinforcement, position of reinforcement and material. In Fig. 6 the effect of these variables on the load and deflection corresponding to the first crack is graphically shown, and the following observations may be made: 1% of reinforcement 1 in. from the bottom of the beam increased the strength of a plain concrete beam from 2 200 to 7 400 lbs., and increased the flexibility from 0.01 in. center deflection to 0.14 in. center deflection. Increasing the reinforcement from 1% to 2% of the cross-sectional area increased the strength from 7 200 lbs. to 10 000 lbs., with only a slight increase in the flexibility. Placing the 1% reinforcement 2 ins. from the bottom face decreased the strength from 7 200 lbs. to 5 000 lbs., with a slight decrease in flexibility.

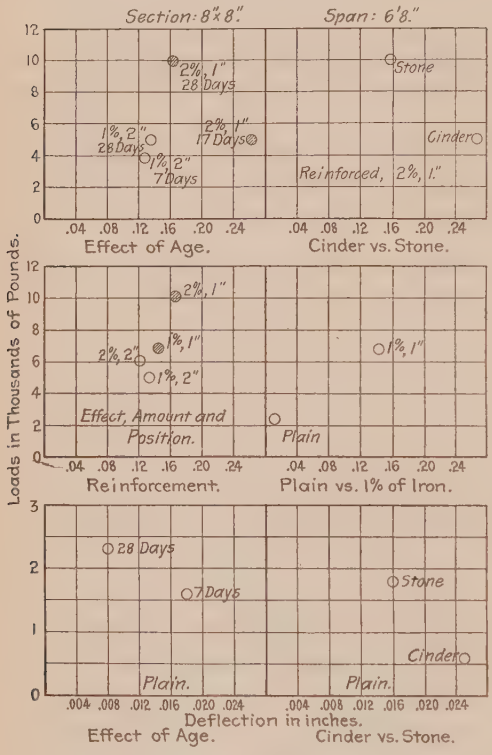



FIG. 6. Diagram showing Effect of Age, Position and Amount of Reinforcement on Load and Deflection Corresponding to the First Crack in Concrete Beams.

A cinder-concrete beam and a stone-concrete beam each reinforced with 2% of metal, 1 in. from the bottom face, had comparative strengths of 5 000 and 10 000 lbs., respectively, and a comparative flexibility of 0.26 in. and 0.16 in., respectively. In case of plain cinder and stone beams, the comparative strength was 600 lbs. and 1 800 lbs., and the comparative flexibility was 0.023 in. and 0.016 in., respectively. It thus appears that reinforcing a beam with even 1% of wrought iron gives it ten times

TABLE VI. (SHOWING SUMMARY OF CALCULATED AND OBSERVED RESULTS FOR REINFORCED CONCRETE BEAMS.)

| Kind of Beam. | | | | | | Load at Point A. lbs | | Load at First crack. lbs | | Load at Failure. lbs | | Stress; steel, lbs. per sq. in. | | Concrete in compress, lbs. per sq. in. | | | Value of x for position of neutral axis. | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Cement. | Sand. | Stone. | Reinforced Per ct. of. | Position. | | Experiment. | Theory | Experiment. | Theory. | Experiment. | Theory. | Point A. | First crack. | Point A. | First crack. | Failure. | Point A. | First crack. | Failure. |
| 1 | 2 | 4 | 1% | 2 ins. | | 2 000 | 2 103 | 5 000 | 4 925 | 5 500 | 5 470 | 2 000 | 17 320 | 457 | 1 556 | 1 699 | .433 | .302 | .272 |
| | | | | | | 2 500 | ..... | 4 300 | ..... | 6 200 | ..... | .... | .... | .... | .... | .... | .... | .... | .... |
| 1 | 2 | 4 | 1% | 1 in. | | 2 500 | 2 266 | 6 500 | 6 232 | 7 300 | 6 705 | 2 700 | 22 000 | 474 | 1 680 | 1 810 | .441 | .318 | .298 |
| | | | | | | 2 500 | ..... | 7 200 | ..... | 7 500 | ..... | ..... | .... | .... | .... | .... | .... | .... | .... |
| 1 | 2 | 4 | 2% | 1 in. | | 3 000 | 2 648 | 10 000 | 9 680 | 12 000 | 12 515 | 2 760 | 21 570 | 516 | 2 188 | 2 710 | .463 | .378 | .382 |
| | | | | | | 3 000 | ..... | 10 000 | ..... | 11 800 | ..... | .... | .... | .... | .... | .... | .... | .... | .... |
| 1 | 2 | 4 | 2% | 2 ins. | | 2 000 | 2 400 | 5 750 | 7 123 | 10 250 | 10 840 | 1 950 | 16 550 | 408 | 1 937 | 3 050 | .454 | .354 | .337 |
| | | | | | | 2 500 | ..... | 6 500 | ..... | 10 250 | ..... | .... | .... | .... | .... | .... | .... | .... | .... |

NOTE.—On account of dead weight of beam the theoretical gross loads should be reduced by one-half this dead weight in order to show true comparison as in Fig. 8.

its former flexibility and more than three times its former strength.

## THEORY.

Having established the facts developed from tests of the beams and their constituent materials, it remains to determine how far the theory, which will be advanced below, accords with the facts, and to recommend constants for use in the design of such beams. In the foregoing it has been the desire of the writer to so present the facts that the reader may make his own deductions should he disagree with those below.

This paper will not take up the development of the theory in much detail. The theory, the main analysis of which is indicated below, intends to account for the three characteristic points of the load-deflection curve shown in Fig. 5, viz, the point A, the load at first crack, and the load at failure.

The assumptions upon which the theory is based are mainly as follows: (1) The cross-sections of the beam remain plane surfaces; (2) the applied forces are perpendicular to the neutral surface of the beam; (3) the values of the moduli of elasticity obtained in simple tensile and compressive tests will apply to the material when under stress in the beams. (With reference to this connection between simple tests and flexural tests, it may be expected that the material in compression at the compressed face of the beam will bear a greater fibre stress than that indicated in a direct-compressive test, for these "outer fibres" of concrete are supported by material which has not reached its maximum strength.) (4) There is no slipping between the concrete and the steel reinforcement; and (5) there are no initial stresses in the beam due to shrinkage or contraction of the concrete while setting. The analysis supposes fracture to be due to bending and not to horizontal shear.

If the cross-sections are assumed to remain plane surfaces during flexure, the distortion of any fibre will be proportional to its distance from the neutral axis. It follows that the law of variation of stress will be represented by the stress-strain diagrams shown in Fig. 5.

Following the method of M. Considère in general by applying these diagrams to the cross-section of a beam, we will have the representation of the stresses at different points of the

cross-section. These stresses in the concrete and that in the steel reinforcement are shown in Fig. 7.

For the sake of simplicity in the analysis, these stress-strain diagrams are assumed to be parabolic arcs, an assumption which is justified in the case of compressive stress-strain diagrams by an examination of a large number of tests recorded in "Report of Tests of Metals for the Year 1899." Referring to this figure, let

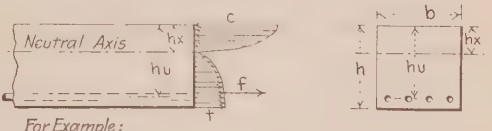



FIG. 7. Diagram Indicating Nature of Stresses in Reinforced Concrete Beam.

l = length of span; b = width of cross-section.
hx = the distance from the compression face to the neutral axis;
hu = the distance from the compression face to the centre of gravity of the reinforcement;
p = the ratio of the area of steel to that of the cross-section of the beam;
$E_s$, $E_c$, $E_t$ = the moduli of elasticity o fthe steel, concrete in compression and concrete in tension respectively;

$$n = \frac{E_c}{E_t};$$

$$m = \frac{E_s}{E_t};$$

f = stress in metal reinforcement;
c = compressive stress in outer fibre of concrete;
t = tensile stress in outer fibre of concrete.

Reinforcement is supposed to be in the tension flange alone.

$E_c$ and $E_t$ are measured at the stresses c and t.

The values of x, u and p are ratios; p and u are at the control of the designer, while x depends on p, u, n and m; n and m are fixed by the quality of the materials, and they change during flexure with the varying values of c, f and t; that is, the modulus of elasticity of the concrete varies with the stress at which it is measured. For practical purposes of computation, however, the constant values of n and m may be used appropriate to the point A and to the point of cracking shown in Fig. 5.

On the assumption of plane cross-sections during flexure, we may determine the ratio of c to t and f to t as follows:

$$c = \frac{t\,n\,x}{1-x}. \qquad (1)$$

$$f = \frac{t\,m\,(u-x)}{1-x}. \qquad (2)$$

Next, to locate the neutral axis; that is, to determine the value of x, we may equate the forces of tension and compression on the cross-section, assuming, as before, that the stress-strain diagrams are arcs of parabolas. Thus:

$$\tfrac{2}{3}\,c\,x = \tfrac{2}{3}\,t\,(1-x) + p\,f. \qquad (3)$$

Inserting the values of c and f obtained above, we obtain the following quadratic:

$$\tfrac{2}{3}\,x^2\,n = \tfrac{2}{3}\,(1-x)^2 + p\,m\,(u-x).$$

Solving the quadratic we have:

$$x = \frac{-\left(\frac{4+3\,p\,m}{2}\right) + \sqrt{4\,n + \frac{9}{4}p^2m^2 + p[6\,m\,(u[n-1]+1)]}}{2\,(n-1)} \qquad (4)$$

Having obtained x, we may compute c and f, and finally obtain the moment of resistance of the section. Taking moments about the neutral axis, we have: M =

$$t\,b\,h^2\left(\frac{5}{12}(1-x)^2 + \frac{5\,n\,x^3}{12\,(1-x)} + p\,\frac{(u-x)^2}{1-x}\,m\right): \qquad (5)$$

No useful development will result from the substitution in Eq. (5) of the value of x, obtained in general terms in Eq. 4. In practical computations n,* m, u and p are given; x is then computed from (4); c and f computed from (1) and (2); finally the moment of resistance is computed from (5).

It may be said that these computations are somewhat tedious, but that it would be practicable for a designer after a few days of work to tabulate all the beams falling in a given range of design. As in the case of problems in hydraulics, diagrams would greatly facilitate computation.

These equations given above are to be applied to compute

*If $n=1$, then $x = \frac{2+3\,p\,m\,u}{4+3\,p\,m}$

the load at the point A. At the load corresponding to the cracking of the concrete in the tension face, these equations should be modified to correspond with the fact that the stress-strain diagram for the concrete in tension is more nearly a rectangle than a parabola. The difference, however, between the results at the time of the appearance of the crack due to the assumption of a rectangle or a parabola is small. With proper values of n and m the equations may be allowed to stand.

When, however, the crack having formed itself, extends throughout the lower region of the cross-section, the equations must be modified by the omission of the effect of the tensile forces due to the resistance of the concrete under tension.

We have then:

$$\tfrac{2}{3} c x = p f \text{ or, } p \frac{E_s}{E_c} (u - x) = \tfrac{2}{3} x^2, \quad \ldots\ldots\ldots\ldots (4)^1$$

which serves to locate the neutral axis. When f is assumed to be the elastic limit of the reinforcing metal c may be computed. As noted above, in the stone-concrete beams tested by the writer, the elastic limit of the iron was reached before the concrete failed in compression.

The resisting moment of the section is then

$$M = b h^2 \left\{ \frac{5}{12} c x^2 + p f (u - x) \right\} \ldots\ldots\ldots\ldots (5)^1$$

## COMPARISON OF THEORY AND EXPERIMENT.

The loads carried by the stone-concrete beams as tested and recorded will now be compared with the loads computed under the application of the foregoing equations, using the elements of strength of the constituents as found by actual test. For this purpose the stone beams at the age of one month will be used. The values assumed are as follows: For the point A, $n = 2$, corresponding to c = stress below 750 lbs. per sq. in., and $m = 12$. The tensile strength of the concrete, t, is 300 lbs. per sq. in.

For the load at first crack, $n = 12$, corresponding to $c = 1\,500$, $m = 90$; t is assumed to be 300, with an elongation of 1 : 1 000. For the point of failure, f is assumed to be 36 000 in Eq. $(4)^1$ and $(5)^1$. The results of this comparison of theory and

experiment are given in Table VI and Fig. 8. The gross loads in Table VI, from theory, are corrected to be net loads in Fig. 8 by subtraction of one-half of the weight of the beam. It may be said in general that the agreement between theory and experiment is more satisfactory than could be expected when the nature of the constituents is considered. It is a well-known fact that two pieces of concrete made under similar conditions do not show concordant tests. This satisfactory agreement in Table VI is partly due to the uniform results obtained from the tests on these beams. It is a fact that the method of loading and support in flexure tests is mechanically more perfect than in case of tension and compression tests.

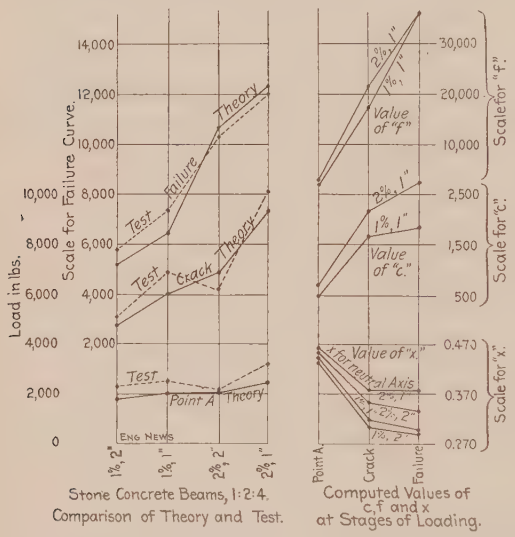



FIG. 8. Diagram showing Comparison of Theory and Test for Reinforced Concrete Beams.

The agreement between tests and theory in case of point A would have been closer if the values of n and m had been slightly increased. The neutral axis rises as the loading increases, and f and c increase, as is shown in the right-hand side of Fig. 8.

## CONCLUSIONS.

While realizing the imperfections in carrying out a somewhat ambitious program, which involved a great deal of labor and observation, and recognizing the fact that many minor points are left without discussion, as for instance the shrinkage stresses arising from rich mixtures, the effect of vibration in loosening the adhesion, the equations for double reinforcement, the writer feels that he is justified in the following conclusions:

1. The nature of the load-deflection diagram for the case of flexure of reinforced concrete beams is established (Fig. 5).

2. The representation of the stresses over the cross-section, as shown in Fig. 7, forms a working basis for analysis.

3. The equations given for the purpose of computing the loads corresponding to the three characteristic points of the load-deflection diagram may be expected to give true results when the proper constants are inserted.

4. In designing 1-2-4 stone-concrete beams the load at point A may be computed using values of n and m of 2 and 12, respectively. The load at first crack in the tension flange may be computed by using values of n and m of 12 and 90, respectively. The variation between theory and experiment was, on the average, nearly 8% when the constants were used.

5. Using these equations and these values of n and m the designer may decide on the factor of safety obtaining in any given design.

6. Indications are given of the effect of varying the materials and age on the strength and flexibility of such beams.

7. The resistance due to tensile forces on the cross-section under the working loads is an important element in the strength of a reinforced concrete beam.

8. Using equations given above the moment of resistance at point of first crack of these 1-2-4 concrete beams with rods one inch from tension face may be computed from formula, $M = K\,t\,b\,h^2$ where k has the following values :

| | | |
|---|---|---|
| For 1 | per cent reinforcement, | $K = 0.84$ |
| For 1.5 | " " " | $K = 1.01$ |
| For 2.0 | " " " | $K = 1.31$ |

t is tensile strength of concrete, about one-tenth the compressive strength. For other mixtures this value of k depends on ratios of moduli of elasticity of concrete and steel.

The experimental values $k\,t = K^1$ for beams tested above are given as follows:

TABLE VII.—EXPERIMENTAL VALUES OF CONSTANT IN EXPRESSION FOR MOMENT OF RESISTANCE OF REINFORCED CONCRETE BEAMS

| Beam. | Reinforcement. Amount and Depth. | Age | $K^1$ in Formula, $M = K^1 b h^2$ |
|---|---|---|---|
| Stone 1–2–4 | 1 %—1 inch. | 1 month. | 266 |
| " | 1 %—2 " | " | 195 |
| " | 2 %—1 " | " | 391 |
| " | 2 %—2 " | " | 240 |
| " | 1 %—2 " | 7 days. | 160 |
| " | None. | " | 58 |
| " | None. | 18 days. | 90 |
| Cinder 1–2–4 | 2 %—1 " | 17 days. | 195 |

The values of $K^1$ in this table require that the dimensions of the beam be expressed in inches and the resulting moments of resistance M at point of first crack will be in terms of inch-pounds.

In the judgment of the writer the safe moments of resistance of these beams may be computed from values of $K^1$ equal to one-third of those quoted in Table VII.

# EFFECT OF VARIATIONS IN THE CONSTITUENTS OF CAST IRON.

By W. G. Scott.

It is proposed to describe in this paper the influence of the metalloids on cast iron as observed under practical conditions, without regard to theory.

The J. I. Case laboratory, where experiments have been made in this line, was established solely for commercial purposes, and no expense has been spared to obtain practical results. This laboratory does the work for five grey iron and three malleable iron foundries, and tests are made on various sizes of both round and square bars. Particular attention has been paid to the influence of the metalloids on cast iron, and from results obtained during a period of five years we are inclined to believe that the old-fashioned 1-inch square test bar is more sensitive to the variations in cast iron than any other. Two of the foundries in particular use this style of bar, one concern uses a one-half inch square bar, one foundry a bar two by one inches, and the others a round bar one and one-eighth inches in diameter.

All of these foundries are mixing by analysis, and have their raw material tested, *i. e.*, pig iron, fuel, etc. A physical test is made on the bars every day and a chemical analysis twice a week. Consequently we have a record of the actual mixture, the physical test and chemical composition of the iron produced by the different foundries, constituting a series of comparisons free from any prejudicial influence.

The most important results as to the influence of the metalloids have been obtained from castings and test bars made by two foundries in particular, which are located in different cities, and make an entirely different line of castings. The only thing in common with these two concerns is their test bars, which are of the same size and similar in other respects, except in the matter of gating. The bars in question are one inch square, cast horizontally, in sand, and run against an iron yoke, the ends of which are exactly 13.333 in. apart. The object of the yoke is to deter-

mine the shrinkage and chill. The shrinkage is measured by placing the bar in a laboratory yoke fitted with a micrometer; deducting one-tenth gives the shrinkage per foot. Three bars are cast in yokes, and one bar in the sand without a yoke. The three yoke bars are used for a transverse test, laid on supports 12 in. apart and the load applied in the middle. The sand bar is used for a tensile test, and is given an allowance of 8 in. between the grips of the testing machine. All bars are tested on a Riehlé automatic screw power testing machine.

In addition to the square bars we also have for test the round bar previously spoken of, but in this case the bar is cast vertically (no yoke being used), and is accompanied with a fluidity strip. Chemical analyses are made from drillings taken from the bars or from blocks made in the mold with the bar. Carbon determinations are made on the bars, blocks, and castings. The ratio of the carbons usually varies according to the size of the casting.

In addition to the transverse and tensile test, we frequently submit the bars to a torsional strain, and to shock, determining the resistance to impact by means of the "drop test." The hardness of the iron is determined by means of the "Bauer Drill Test" and an electrical register.

In a general way the physical tests applied to cast iron are those relating to the practical features most desired by the manufacturer, viz:

Good strength, with a close grain.

Material not too hard to machine.

Shrinkage to correspond with the pattern.

In addition to this, the manufacturer expects the iron to be free from blow holes, cold shuts, scabby iron, etc. Some of these defects may be eliminated by manipulating the constituents of the mixture, others must be corrected in the molding.

In most cases where the iron is not mixed by analysis, every defect or poor batch of castings is blamed upon the pig iron or coke; on the other hand, when mixing by analysis, the molder is the party to suffer, especially if the chemist is running the foundry. In the event of the foundryman and the chemist working in harmony, it is not a difficult task to get at the truth of the matter, and soon remedy such defects. It is customary

for founders mixing by analysis to adopt certain mixtures for their various grades of cast iron, such as a "Cylinder Mixture," "Pulley Mixture," etc., or to classify the iron as, Soft, Hard, and Medium.

No general rule can be given for the exact content of silicon, sulphur, etc., that will produce precisely the same iron in different shops, as will be shown by the following example:

Mr. A. makes his cylinder mixtures on the formula of 1.50% of silicon and leaves the cylinders in the sand to anneal; the result is a *soft* close-grained iron. Mr. B. copies the formula, but uncovers the castings as soon as possible, because he is in a hurry or does not have floor space; the result is an iron too hard to be machined. Now, if B. had made his cylinders with 1.80% of silicon, he would have obtained the proper iron. This goes to show that the main controlling element in iron is carbon. Carbon, however, is influenced by the other metalloids, and the amount of these constituents present will govern the final result, as shown by the strength, deflection, shrinkage, chill, etc.

*The Influence of Silicon.*—The metalloid *silicon* exists in three allotropic modifications, one amorphous and two crystalline. Silicon was first obtained by Berzelius in 1823 from the silico-fluoride of potassium by the action of fused potassium thereon. It appears, when thus obtained, as an amorphous (*i. e.*, non-crystalline) powder of a dull brown color, which smears the fingers like plumbago.

One form of the crystalline silicon resembles graphite, both in regard to its flaky form and dark color. The other crystalline variety is known as "diamond silicon," and crystallizes in the form of needles, having a steel-grey color. Heated without free access to the air, the *amorphous* silicon is changed into the crystalline variety and heat is developed; heated otherwise it readily burns until it becomes coated with oxide, which prevents further oxidizing. The *crystalline* silicon does not burn at all, not even at a white heat, in pure oxygen, but, strangely enough, rapidly burns at a cherry-red heat in carbonic acid gas. When metallic silicon (Si.) is converted into oxide it becomes a white powder and is called *silica* ($Si O_2$). Oxide of silicon or silica, is nothing more or less than pure sand.

As to the influence of this peculiar metalloid on cast iron, we may mention that its effect on carbon is such, that between certain points the iron is either soft or hard, the degree of hardness increasing or decreasing with the amount of silicon present. In a general way, silicon from 1.00 to 4.00% in cast iron acts as a softener, below 1.00% it begins to harden and produce "chill," the degree of hardness and chill increasing in given ratio as the silicon decreases. Above 4.00% it begins to harden and takes on the peculiar property of what is called "rotten shortness." The higher the silicon the more short and weak the iron will be. As an example, a pig of No. 2 iron with about 2.00% of silicon is hard to break with a sledge hammer, whereas a pig of ferro-silicon with about 8.00% of silicon is easily broken by letting it fall on another piece of iron.

With the other metalloids constant or in a normal condition, cast iron will attain its maximum of softness with silicon between 2.30 and 3.50%, and a very remarkable feature in this respect is that the average point between, or 2.90%, produces an exceptionally soft iron. High sulphur destroys the effect of silicon as a softener, one point (.01) of sulphur being equal to about 15 points (.15) of silicon in this respect. (See sulphur.)

The requisite amount of silicon in castings to produce a product that will machine easily depends on several conditions, *i. e.*, the thickness of the casting, the time of cooling, and the desired grade of iron. For small thin pulleys, the silicon should range from 2.30 to 2.80%. For medium-sized castings it may run from 2.00 to 2.50%, and for large work from 1.50 to 2.00%.

The time of cooling has a great influence on the softness of the iron, especially in the case of low silicon. For instance: a casting with 1.50% of silicon, left in the sand until cool, will be as soft as a similar casting with 1.80% which is uncovered and allowed to cool quickly. Castings of medium size, with as low as 1.00% of silicon, providing the carbons are not too low, may be easily machined if the material is annealed at a moderate temperature for several hours. As a usual thing the majority of the foundries shake the castings out of the sand as soon as possible, owing to lack of floor space, etc., in which case an

allowance must be made for a little higher silicon than that called for in an ideal mixture.

Chill is greatly influenced by silicon and is in a measure entirely dependent upon the amount present. Low silicon gives an increased chill, while high silicon acts in a directly opposite manner. With silicon above 1.25%, the other metalloids being normal, there is little or no chill in medium and large-sized castings. The per cent. of silicon calculated to produce a given chill may be approximated as follows:

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| 1.00 | per | cent | of | silicon | is | equivalent | to | 1-8 | in. | chill. |
| .70 | " | " | " | " | " | " | " | 1-4 | " | " |
| .52 | " | " | " | " | " | " | " | 5-8 | " | " |
| .40 | " | " | " | " | " | " | " | 1 | " | " |
| .30 | " | " | " | " | " | " | " | 1½ | " | " |

Carbon, sulphur and manganese are strong factors in regulating the chill; total carbon low, sulphur high, and a given amount of manganese, can increase chill in a wonderful manner. With total carbon and manganese normal, an increase of sulphur will make a radical change in the depth of the chill. In some foundries making chilled work it is customary to introduce a small amount of iron sulphide to obtain a given chill. A high carbon will give a hard, shallow chill, while a low carbon gives a soft but deeper chill. Chill is governed to a great extent by the cooling of the metal, suddenly cooled iron taking more chill than an iron cooled slowly; hence the difference in using a large or small chill block. Pouring the iron vertically or horizontally also gives different results.

Shrinkage is the one particular feature in cast iron which depends more on the influence of silicon than any other metalloid, and it is possible now to govern the shrinkage in a practical manner for commercial purposes. *Silicon decreases shrinkage* in a given ratio, and as nearly as can be determined by actual practice, every 0.20 % of increase in silicon is equivalent to about 0.01 in. per foot decrease in shrinkage. Sulphur has a stronger influence and increases the shrinkage more than silicon decreases it, but does not have so wide a range; hence, unless the sulphur is abnormally high, the silicon is the controlling element. In depending upon silicon to decrease shrinkage, attention must

be paid to the other metalloids present; thus, with high phosphorus, which also reduces shrinkage, the result is proportionally larger. Manganese with a high silicon helps to reduce shrinkage by neutralizing the effect of sulphur, but when high enough to change graphite into combined carbon it acts slower, and in some cases just the reverse.

A very noticeable feature about the shrinkage of cast iron is that a bar taken from the first part of a heat has generally a greater shrinkage than one from the latter part of the heat, the silicon being the same; this is due to a difference in the carbons, blast, temperature, etc. High silicon iron is very fluid when in a molten state; it is necessarily very hot, as every 1% of silicon that burns causes a rise in temperature of about 570° F., consequently contraction must be greater than in a low silicon iron.

The loss of silicon in the cupola will generally range from 0.15 to 0.25%, or an average of 0.20%. In calculating mixtures it is the rule to subtract 0.20% of silicon for loss due to combustion or oxidation. This 0.20% is subtracted from the final result; thus, if the computed "estimate" is 2.65%, the "actual" amount in the castings would be 2.45%. The maximum hardness of cast iron is obtained with silicon under 0.80%, medium hard at about 1.90, and very soft at 2.90%. At 4.00% it begins to harden somewhat, increasing slightly as the silicon rises, and becoming quite hard at 8.00 to 10.00%.

In regard to *strength*, silicon plays an important part, very low or extremely high silicon imparting less strength than the intermediate amounts. The following table illustrates the influence of silicon on strength:

| Per cent. of Silicon. | | Transverse Strength. | Tensile Strength. | |
|---|---|---|---|---|
| 0.20 | = | 2 600 lbs. | 20 500 lbs. | Extremely hard. |
| .50 | = | 2 800 " | 24 500 " | Very hard. |
| 1.00 | = | 3 000 " | 25 500 | Hard. |
| 1.40 | = | 3 300 " | 26 500 " | Medium hard. |
| 1.90 | = | 2 900 " | 31 500 " | Medium soft. |
| 2.20 | = | 2 500 " | 30 000 " | Soft. |
| 2.50 | = | 2 300 " | 29 000 " | Very soft. |
| 3.00 | = | 2 000 " | 24 500 " | Extremely soft. |
| 3.50 | = | 1 700 " | 22 500 " | Slightly hard. |

These figures are only approximate, the two lower silicons

are doubtful, but from 1.00% up the results are fairly accurate as an average of many tests, and agree closely with other experimenters. It will be noticed that the highest transverse strength is with silicon at about 1.40%, and the highest tensile strength at 1.90%. It is almost impossible to obtain accurate figures on strength as influenced by silicon, owing to the constant change in the carbons.

Likewise a change in the manganese, phosphorus and sulphur makes a great difference, yet with manganese at 0.50, phosphorus at 0.70, and sulphur not above 0.085% (normal conditions for Southern coke irons), or with phosphorus at about 0.40 in Northern irons), it may be safely said that the best results in transverse strength are obtained with a silicon content ranging from 1.30 to 1.50%, and the highest tensile strength between 1.60 and 2.00%.

In conclusion, *silicon softens iron, imparts fluidity, decreases shrinkage, opens the grain, promotes the formation of graphite carbon, and reduces the strength.*

*The Influence of Sulphur.*—Sulphur has a great affinity for iron, and unites with it to form sulphide of iron. Iron pyrites, a gold colored metal, with the formula $FeS_2$, contains about 53.33% of sulphur and 46.67% of iron. It is intensely hard, but somewhat brittle. Ferrous sulphide, or sulphide of iron, as it is more frequently called, has the formula FeS, and contains about 36.36% of sulphur and 63.64% of iron. It is of a dull black color, very hard (not so hard, however, as pyrites) and very brittle; in fact, is "rotten short."

When pig iron is melted in the cupola it not only retains its original content of sulphur, but absorbs considerable more from the fuel. The amount of sulphur absorbed by iron in melting depends upon certain conditions, *i. e.*, the time the molten iron is in contact with the coke; the degree of temperature; the amount and quality of the flux; the blast, and the size of the cupola. Most of the foundries at the present time use coke for melting, especially is this true in cupola practice, although we occasionally find some using a mixture of coke and coal. Foundry coke usually carries from 0.50 to 1.50% of sulphur, Lehigh coal ranges from 0.30 to 0.90%, and Splint coal from 0.30 to 2.50%.

All foundrymen desire a low sulphur coke, but as a rule the very low sulphur cokes are light in structure; consequently when structure is taken into account it will be found that the strong cokes will seldom be below 0.75% in sulphur, even in coke made from washed coal. In melting iron in a cupola a large amount of the sulphur from the fuel is carried off as calcium sulphide in the slag, the calcium (lime) of the limestone flux having quite a strong affinity for sulphur. A high temperature favors the expulsion of sulphur by driving it into the slag; on the other hand, a low temperature has a tendency to make it unite with the iron. If much manganese be present still more sulphur will be eliminated. Silicon also has a tendency to throw out some sulphur owing to the fact that it raises the heat of combustion.

In the average cupola, lined up to, say, a diameter of 60 or 62 in., and working under favorable conditions, the sulphur absorption will vary from 0.03 to 0.05%, but more often as 0.05%. To illustrate this, we will suppose that the coke analyzes 0.70% of sulphur. Now, 0.05% of 0.70 is 0.035%, the amount which must be added to the initial sulphur in the iron.

In estimating the sulphur in a mixture which figures out 0.055% of sulphur for the pig, the castings will have 0.055 plus 0.035 or 0.090% of sulphur. In many reverberatory furnaces the absorption figure is often less than 0.03% of the sulphur in the fuel. Sulphur hardens iron, one point, or 0.01%, offsetting about fifteen points, or 0.15% of silicon as a softener. It not only makes the iron hard, but produces both "red shortness" and "cold shortness" if very high.

Sulphur up to a certain point increases the strength, the greatest strength in cast iron fit to use being at about 0.150% for low phosphorus iron, and at 0.095 for a medium high phosphorus iron. One point (0.01) of sulphur is equivalent to about 50 lbs. increase of strength. This strength is obtained by the sulphur changing the graphitic carbon into the combined form, therefore cannot be entirely credited to sulphur. For frictional wear, high sulphur iron gives very satisfactory results, as it closes the grain and increases hardness.

Sulphur increases shrinkage and chill, its energy in this respect being great. Every 0.03% increase in sulphur corresponds closely to .01 in. per foot increase in shrinkage. It is,

however, quite impossible to calculate shrinkage by sulphur, owing to the fluctuation of the carbons.

Chill is induced by sulphur, and as before stated, some founders introduce sulphide of iron for this purpose, but it cannot be recommended, as it produces a brittle chill, and stamp-shoes for mining machinery made in this way, which at first gave good satisfaction, soon went to pieces, probably from the effect of repeated shock.

Sulphur gives rise to "blow holes;" it also favors "dirty iron," especially when the manganese is low. By increasing the manganese the evil effects of sulphur are neutralized and the blow holes almost completely eliminated. Sulphur in cast iron may be pretty high if sufficient manganese be present; with manganese at 0.30 the sulphur should not exceed 0.065%, but with 0.60% of manganese an iron with 0.095% of sulphur will produce good castings, while a fair grade of cast iron may be produced which contains 0.75% of manganese and as high as 0.130% of sulphur.

Sulphur with high phosphorus produces a weak, brittle iron, in which it will be noticed that the shrinkage is not excessive, but that the chill is entirely different in nature, and that the deflection in a transverse break is at a minimum. In the case of low sulphur, phosphorus increases the strength somewhat. Sulphur increasing, and phosphorus decreasing shrinkage, it is evident that when the phosphorus is high, a high sulphur iron will be slightly softened.

Sulphur has a powerful influence on carbon, changing the graphitic into the combined form.

With a low total carbon, sulphur is likely to change all or nearly all of the graphite into combined carbon; this is often the reason why a high sulphur iron made into thin castings produces hard white iron. With an extremely high total carbon a strong, close-grained iron is the result with high sulphur, but with high silicon, the strength is reduced and the bar breaks "dead," that is, there is no snap to the metal when it breaks.

The influence of sulphur to keep the carbon in the combined state is greater than that of silicon to make graphitic carbon; hence the strain in iron with excess of sulphur, carbon and silicon. Sulphur has the property of driving out carbon, and if the sulphur

be high enough the carbon will be almost completely driven out, as iron sulphide and iron carbide seldom unite. Sulphur lowers the melting point of iron, but the molten iron is generally "sluggish" and congeals quickly, thus enclosing escaping gases and thereby forming blow holes. The consistency of molten iron high in sulphur, tends to prevent the dross, kish, etc., from rising to the surface; hence *dirty iron.*

With sulphur and phosphorus low and silicon not too high, castings may be produced which will bend to quite an extent, and which may even be punched. From this it may be inferred that a low sulphur and phosphorus, with other metalloids normal, will produce an iron with a high deflection.

In conclusion, sulphur may be termed a very detrimental mental element, and one that *makes the iron hard, increases shrinkage, promotes chill,* causes the iron to *congeal quickly,* and gives rise to blow holes, shrinkage cracks, dirty iron, etc.

*The Influence of Phosphorus.*—The special features claimed for phosphorus are, that it makes the iron fluid and decreases shrinkage. Above 0.80%, phosphorus begins to impart "cold shortness," *i. e.,* makes the iron brittle or short when cold. Above 1.20% it begins to harden the iron by causing the graphitic carbon to assume the combined form, and above 1.60% it is so "hard short" that a very light shock will break the castings.

The more combined carbon present, the lower the phosphorus must be; thus, in chilled work the phosphorus should be kept down very low, say under 0.30%, where the combined carbon is liable to be 1.00 or 1.50%. In malleable castings with low silicon it is customary to keep phosphorus below 0.20%, in fact the lower the better. For ordinary grey iron the limit of safety is placed at 0.70%. For thin, soft castings, where strength is of minor importance, the phosphorus may be allowed to run up to 1.00 or 1.25%; such a mixture will run easily and fill every part of the mold.

High phosphoric iron, on account of its great fluidity, readily permits the dross to rise to the surface; therefore the iron is seldom dirty. High phosphorus with high silicon produces an extremely fluid iron, but it has very little strength.

If the silicon be low, then the phosphorus will affect the carbons, changing the graphitic carbon into the combined, in which case there will be an increase of strength.

Phosphorus and sulphur working in opposite directions, *i. e.*, by decreasing and increasing shrinkage, will produce a hard brittle iron, which will be very susceptible to internal strains, and result in chill or contraction cracks. High phosphorus and low sulphur seem to go well together. Phosphorus with high manganese gives a hard iron with very little deflection, a fact due to increase of combined carbon. In certain cases where silicon is high and sulphur low enough not to materially favor combined carbon, a high manganese will greatly offset the cold shortness due to high phosphorus.

In regard to shrinkage, it may be assumed that one point (0.01%) of phosphorus is about equal to three points (0.03%) of silicon in its power to reduce shrinkage. Cast iron intended to withstand high temperatures should be low in phosphorus and sulphur, and if combined carbon be low and graphite high, the effect is stronger still.

In making mixtures by analysis no correction is made for phosphorus, as there seems to be neither a gain nor loss in the cupola, and the small amount of phosphorus in the coke is too small to be taken into consideration in grey iron mixtures.

As a rule, it may be taken for granted that *phosphorus lowers the melting point of iron, imparts fluidity, decreases shrinkage, weakens the iron, and makes it "cold short" or brittle under impact* (*i. e.*, shock).

*The Influence of Manganese.*—Manganese tends to keep the carbon in the combined state, hence it causes the casting to be white, unless its influence is counteracted by high silicon or an excess of graphite. Large castings, remaining fluid longer and cooling slower, will bear more manganese than small ones. Large castings may contain as high as 1.00%, medium-sized, 0.60, and small thin work 0.30%, without detriment. The normal average manganese content in grey iron castings is about 0.40%. With high silicon (2.30 to 2.80%) large castings may contain 1.25 and small ones 0.60%, without being hard. Large castings with manganese above 1.40, or small ones at and above 0.70%, begin

to harden, and the thin parts are inclined to become white. With low silicon the whole of the iron is liable to be white, especially if cooled quickly.

Manganese has a powerful effect on carbon, changing graphitic carbon into the combined; hence it increases strength. shrinkage and chill. It likewise stiffens the iron, consequently reduces deflection. Sulphur in changing graphite to combined carbon produces a hard iron with a very close grain, whereas manganese produces an iron with a slight crystalline structure. Manganese has a very beneficial effect on high sulphur iron, neutralizing the bad effects due to brittleness, hardness and shrinkage.

Iron melts at a temperature of 2 000° to 2 300° F., manganese irons from 2 200° to 2 500° F.; consequently a high heat is necessary to make it combine with iron. A hot working furnace will send the manganese into the iron, while a cold working furnace will drive most of it into the slag. When ferro-manganese is charged along with the iron in a cupola it has an entirely different action than when added to the molten metal in a ladle. In the cupola much of the manganese is lost by oxidation and passes off in the slag, carrying with it a large amount of impurities, if there be an excess of flux present.

Manganese eliminates sulphur by forcing this element into the slag, providing the heat of combustion is sufficient to reduce the manganese; otherwise the manganese is oxidized itself and passes off with the limestone flux, giving a *brown* slag, while a *greenish* colored slag indicates that the sulphur is going into the slag, and consequently the manganese into the iron.

In calculating the loss of manganese in making mixtures by analysis it is customary to figure a loss of from 0.10 to 0.30%. If the manganese is in the pig iron in its original state, the loss is about ten points, *i. e.*, 0.10%, but when 80% ferro-manganese is charged in the cupola it is customary to figure the loss at 0.20 to 0.25%; in some cases 0.50%.

In adding ferro-manganese to a cupola charge it will be noticed that the resultant cast iron contains somewhat less sulphur, will be correspondingly softer, have more strength and be free from blow holes. In adding manganese in the ladle the ferro-manganese is broken up into small pieces and placed on the

bottom of the ladle, then the molten iron run in, stirring with an iron rod if necessary. Now in this case very little sulphur is expelled, although a slight sulphurous odor is sometimes observed, yet the manganese exerts itself, making the iron much stronger, cleaner, and free from blow holes. In a practical way, using a ladle, holding say, 1 800 lbs. of molten metal, every one pound of 80% ferro-manganese used will raise the manganese in the castings about 0.045%. The usual amount charged is governed by the amount desired in the castings; thus, if an analysis shows the castings to contain 0.25% of manganese, and we desire 0.52, we will add six pounds of the ferro-manganese. Charging a moderately high manganese pig (1.00 to 1.50%) in the cupola gives better results, and is more economical than using ferro-manganese either in the cupola or ladle.

In regard to strength, manganese under 1.00% in cast iron gives the best results; above 1.25% it appears to weaken the iron somewhat by making it too brittle, that is, for commercial purposes (castings, not test bars). When manganese falls to below 0.25% in ordinary soft cast iron (1.80 to 2.50% of silicon) the material is not only weak, but there is generally more or less trouble with blow holes, dirt, etc. On account of the influence of manganese on carbon by hardening iron, it is very necessary that the silicon be high, in order to obtain soft castings. Iron inclined to be hard from high sulphur is greatly softened by increasing the content of manganese, the one neutralizing the effect of the other. Manganese has no direct effect on phosphorus, but when the two combined are high the result is a hard, brittle metal. Manganese generally causes a slight loss of silicon, probably due to oxidation, but the loss is so small that it may be ignored in calculating mixtures.

In conclusion, *manganese above a certain point hardens the iron, increases shrinkage and chill, reduces deflection*, and changes graphitic carbon to the combined. Within certain limits, *it softens iron by neutralizing the effect of sulphur, decreases shrinkage and chill* (from same cause), *increases the strength*, and improves the metal by eliminating blow holes due to occluded gas.

*The Influence of Carbon.*—The affinity of iron for carbon is so great that all technically produced iron contains carbon,

ranging from a few hundredths of one per cent. to five or six per cent. In the fluid state iron may contain much more carbon, but we have no definite figure as to how much. Iron absorbs carbon from the fuel, but in order to unite iron and carbon it is not absolutely necessary to have the iron in a fluid state, as is shown in the case of blister steel, where the bar iron is heated sufficiently long in a packing of carbon. For somewhat similar reasons iron may be forced to give up a large part of its carbon without being fluid, as in the process of making malleable cast iron. However, the union of carbon and iron is more emphatic when the iron is fluid, and the saturation point for carbon will depend upon the amount of fuel, the temperature, the blast, and the various other metalloids in the iron.

Manganese favors the absorption of carbon, while silicon reduces the power to absorb carbon. With much manganese present the iron may contain as high as 6.00% of carbon, but ordinary pig iron with manganese under 1.00% seldom contains more than 4.50%, the general average being about 3.50%.

Pig iron rich in carbon may be made by raising the temperature of the blast and increasing the fuel in the burden. In the ordinary foundry pig irons the highest total carbons are found in iron running from 2.50 to 3.50% of silicon, especially coke irons. Some charcoal irons with a low silicon contain much carbon; this is but seldom true of the coke irons.

Carbon is present in iron in two or more distinct forms, but only two variations have been investigated sufficiently for commercial purposes, viz, free or uncombined carbon, generally termed *graphitic carbon* or graphite, and *combined carbon*, a form in which the carbon is united chemically with the metal to form carbide of iron. The ratio of the two forms of carbon regulates the grade of iron, and the metalloids, silicon, sulphur, phosphorus and manganese, in conjunction with temperature, govern the proportion of the two different forms. Temperature as a rule has more to do with changing the form of the carbon than anything else, but even this is held in restraint to a great extent by the metalloids.

In a pig iron containing a total carbon of 3.50%, the other

metalloids being about normal, the ratio or standing of the carbons will usually be as follows:

| | | |
|---|---|---|
| Combined carbon | = | 3.00 per cent. |
| Graphitic carbon | = | .50 " " |
| Total | = | 3.50 " " |

Combined carbon is the hardening element, graphite the softener. Combined carbon hardens the iron, increases shrinkage and chill, lowers deflection, imparts stiffness, closes the grain, and increases the strength. Graphitic carbon is free graphite, present in the iron as a mechanical mixture; hence it materially weakens the metal. It usually opens the grain, causing the iron to be highly crystalline; it reduces shrinkage and chill, makes the iron soft, and in some cases causes a rise in deflection. Combined carbon lowers the melting point of iron, graphitic carbon raises it; consequently a white iron will melt easier than grey iron. When molten iron is quickly cooled or chilled, more or less of the graphite is changed to combined carbon.

For instance, cast iron with a total carbon of 3.50%, might by slow cooling contain 3.00% of graphitic and 0.50% of combined carbon; on the other hand, if chilled, it could contain 0.50% of graphitic and 3.00% of combined carbon. If much silicon were present then it would be a difficult matter to make such a radical change, but with a high manganese there would be very little resistance.

In melting pig iron in a reverberatory furnace there is generally a loss of carbon, in some cases a large amount, but in the cupola the result is different, there being sometimes a loss, while at other times there is a decided gain. An excess of fuel and a high blast increases the carbon; an oxidizing flame, rusty scrape, etc., reduces it. The longer the iron is in contact with the fuel the more carbon and sulphur it will take up. This accounts for the uneven results shown in tests of iron taken from different parts of the heat. Iron melted and poured very hot will usually have a lower combined carbon than one poured dull, owing to the fact that the hot iron will be longer in cooling in the mold. The richer the iron in carbon, the more liability to separation of graphite in solidifying. Evidently iron in the fluid state contains

only combined carbon, because if it contained graphite this would come to the surface, being so much lighter than iron.

The formation of "*kish*" is due to the fact that the hotter the iron the more carbon it can contain, consequently when cooling the excess of carbon must be forced out, and the kish (*i. e.*, free graphite) being lighter, comes to the surface, and in some cases rises up in a cloud from the ladle, covering everything around with a coating of fine dust or graphite. Kish is very liable to occur in high silicon, carbon irons, containing a low percentage of manganese. Kishy iron is generally very weak and the castings are full of dirt. (Note: The word "dirt" is the foundry term for mixtures of slag, oxide, graphite, etc.) When manganese is high there is seldom any trouble from kish, but manganese pig is not always at hand, in which case the evil may be prevented by adding about 50 lbs. of scrap steel or wrought iron to every 2 000 lbs. of pig iron and scrap in the charge. A large percentage of scrap, either foreign or home, will reduce the excess of carbon in most cases. Shot iron will do the same thing, and high sulphur will restrain it, but the safest way is to add the steel scrap.

In recapitulation, it may be said that, *combined carbon closes the grain of the iron, increases shrinkage and chill, and raises the strength.* The maximum of hardness is with combined carbon at about 2.00%, and the maximum strength at a medium of the combined and graphitic carbon. Graphitic carbon *weakens the iron, reduces shrinkage and chill, and makes a soft iron*, which works well under the tool, unless exceptionally high, when it will be found impossible to obtain a smooth finish.

*Concluding Remarks.*—This paper would hardly be complete without a description or illustration of what are termed "special mixtures" for gray iron castings.

Every foundryman and every chemist making mixtures by analysis has a preference for certain combinations, and naturally uses a mixture giving the best results under existing conditions; thus, if the castings are to be partly annealed or allowed to cool slowly, he will generally figure the silicon as low as possible, or to a point where extreme hardness does not interfere with the machining. Occasionally it happens that in a given mixture or

grade of iron more strength is desired, or a closer-grained iron wanted, without materially changing the silicon content; this effect may be obtained by changing some of the other constituents.

For Ammonia Cylinders (ice machinery), Air Compressors, etc., the following mixture is taken as a standard:

Silicon, between 1.20 and 1.60%, where castings are allowed to cool slowly, or are annealed. If not annealed, then silicon should be between 1.60 and 1.90%. Sulphur should not exceed 0.095%, on account of hardness, but with a high total carbon it may run up to 0.150% without detriment, although with high sulphur there is always a chance of hard iron and excessive shrinkage, especially in low silicon mixtures. Phosphorus should be kept below 0.70%, and if great strength is wanted, it should be much lower, say 0.40. Manganese should not exceed 0.60%, except in case of high sulphur, when it may rise to 0.80%.

The above mixture will produce a hard, strong, close grained iron. In some cases where the carbon is extremely high, it will be found necessary to use considerable scrap in order to reduce the open grain, some foundries using steel scrap for this purpose, others using home or foreign cast scrap.

When a leaky cylinder occurs it is not always necessary to discard it on this account, as it is an easy matter to remedy such defects. A solution of iron chloride, made by dissolving iron drillings, etc., in muriatic acid until the acid fails to dissolve more iron, is poured into the cylinder, or the cylinder washed over with the solution; then ammonia water is introduced, and steam or air pressure applied, driving the iron hydroxide into every pore. When dry the cylinder will be tight.

Medium iron for Engine Cylinders, Gears, etc., may be made on the following formula:

Silicon, 1.40 to 2.00%. 1.50% silicon gives the best results for gears, and 1.60% for steam cylinders.

If the castings have thin parts and have to be shaken out of the sand quite soon, the silicon should be raised ten or twenty points.

Sulphur should generally be kept below 0.085, but if the manganese be high (0.60 or 0.70) the sulphur may be allowed to

run up to 0.100%. The lower the sulphur the softer the iron. Phosphorus should not exceed 0.70%. Manganese should be between 0.30 and 0.70%.

Soft iron for Pulleys, Small Castings, etc.: Silicon should be between 2.20 and 2.80%. The ideal mixture is 2.40% of silicon. Sulphur should not exceed 0.085%. If above 0.095 there is liable to be trouble with hard iron and shrinkage. Phosphorus may be allowed to run up to 0.95% in small thin work, but if strength is wanted it will be advisable to keep it down below 0.70. Manganese may be between 0.30 and 0.70.

It is quite impossible to give the exact strength of the iron for each mixture, but the approximate transverse strength of the "Hard Iron" should be 2 800 lbs., the "Medium Iron" about 2 500 lbs., and the "Soft Iron" about 2 200 lbs.

The strength in any one of the irons may be raised by increasing the manganese content, or lowering the graphite. Thus in the "soft iron" with manganese at 0.30% and a transverse strength of 2 200 lbs., a strength of 2 600 lbs. will be obtained with manganese at 0.70. For all purposes (in soft iron) manganese between 0.40 and 0.60% gives the best results. For obtaining specified results the following points may be considered, any one of which will produce the desired effect.

*To raise the strength:* Reduce the amount of graphite. Reduce the phosphorus. Reduce the silicon. Increase the manganese. Increase the combined carbon.

*To reduce shrinkage:* Increase the silicon. Increase the graphitic carbon. Increase phosphorus. Reduce the sulphur.

*To prevent blow holes:* Reduce the sulphur. Increase the manganese.

*To prevent kish:* Reduce the graphite by increasing scrap, or raise the manganese.

In conclusion, a few facts in regard to test bars, etc., deserve mention.

A square test bar, cast flat, will show many more defects, such as blow holes, shot, dirt, etc., than a round bar cast vertically; furthermore, the transverse strength will be increased from two to three hundred pounds if broken with the gate side up. Tumbling the bars, either round or square, increases the

strength from one to three hundred pounds. Pouring hot iron into damp sand also increases the strength. Deflection increases with the strength unless the bar be extremely stiff from the presence of high manganese, or cold short from excess of phosphorus. Cold shuts are more of a physical defect than due to chemical composition; consequently, like scabby iron, they must be considered in the molding.

In making mixtures by analysis it is customary to allow the foundryman or mixer a leeway of 0.10 to 0.15% on silicon, and 0.005 to 0.010% on sulphur. The moment the scrap exceeds one-half of the total mix, then the unknown quantity exerts itself; consequently foundries using much scrap should be willing to accept results within the above-named figures.

## DISCUSSION.

Mr. Moldenke

R. G. MOLDENKE.—In the first part of his paper Mr. Scott states that the one-inch test bar has given about the best satisfaction all round, and at the end of the paper he states that the one-inch test bar shows more defects and gives different results and that for this reason we should not use the one-inch test bar.

I have made probably 85 000 to 90 000 tests with one-inch test bars on malleable cast iron, and probably 10 000 tests on gray iron; and I find, that while this bar gives me all the needed information for my own shop practice, it is not a thing to recommend generally. We want something more natural, that will give truer information on the subject of iron than the one-inch test bar can. We want something more independent of the conditions that exist in the mold, of the pouring temperature, and of the method of cooling, etc. In order to eliminate these variables, as far as possible, we must increase the size of the bar. That is why the American Foundrymen's Association in their work along these lines has gotten away altogether from small test bars and selected as large ones as they could, consistent with the testing machinery.

Mr Whitney.

ASA W. WHITNEY.—Metallographic methods appear to be exceptionally valuable in the study and testing of white and mottled irons. Whereas, in the case of gray irons ordinary chemical analysis distinguishes two principal forms of carbon—graphitic and combined—it does not in the case of mottled or white iron indicate how much of the combined carbon may go to strengthen and how much to harden. The principles of metallography now permit of fairly correct deduction of this matter, that is, the amounts of pearlite and cementite, from the figures of a complete analysis. But by the actual practice of metallography these and other important details of composition and structure are promptly ascertained, the preparation of a sample requiring but a few minutes.

The further study by metallography of such white iron test bars as I use in the regulation of chilling mixtures (see "Transverse Tests of Chilled Car-Wheel Metal," *Journal of the Franklin*

*Institute*, April, 1897), will, I believe, throw such light on the effects of small changes of composition as to gradually allow omission of some of the more cumbersome physical tests. What Mr. Scott says in regard to certain changes in the composition of cast iron having a strengthening effect and others a weakening effect is of course true only within rather narrow limits. What appears to me more vitally interesting is the fact that very minute but complex alterations of a calculated charge of cast iron can be definitely counted upon to produce definite modifications on any one class of mixture. This is not usually a direct change, but a result of slight changes in relative rates of oxidation of some elements. Mr. Whitney

PAUL KREUZPOINTNER.—I believe it to be a source of gratification to this Society that cast iron is recognized to the extent it now is, and I hope it will continue so in the future. Cast iron is but a child of the same family as iron and steel; and while it is not a twin-brother of steel, it is very closely related. In its cast condition steel is merely a rock formation, and so is cast iron. The time is coming when the study of cast iron will lead to a better understanding of cast steel, and through cast steel we shall come to an examination of the steel ingots used for rolled and hammered steel. When that time shall have come, when the study of cast iron—the microscopic study of cast iron—shall have led us that far, then the steel man will examine his steel ingots with more interest than he does to-day, and many an inferior material will not be put on the market, because of the knowledge gained originally by the study of cast iron. While this may be premature—and I know that sometimes I have been a pioneer in some points in metallurgy—at the same time it will be a natural outcome: that the study of cast iron, of cast steel, and steel ingots will lead to improvement in the finished material. Mr. Kreuzpointner.

THE PRESIDENT.—It has been our custom in Altoona for a number of years, to examine each new pig iron that was brought to our attention for use in the car wheel mixture, by making first a transverse test of test bars cut from the pig, a chemical analysis, a chill test by melting the metal in the foundry, and a transverse test of test bars cast from the remelted metal. In the course of this work it has been our habit to predict from the analysis, what the depth of chill would be. The President.

The President. Sometimes the prediction would be found to coincide very closely with the actual facts as obtained by test, but frequently the depth of chill would differ quite widely from what we would expect from the analysis. We put a good deal of study on this subject, to see if we could find out why, but as yet have not succeeded in locating the cause of the discrepancy. Perhaps some of those who are present can throw some light on this subject.

Mr. Moldenke. MR. MOLDENKE.—I made an experiment once which gives a little light on this subject. It occurred in handling a malleable mixture with .75% silicon, .20% phosphorus, .17% manganese, and .02 to .03% sulphur. I wanted to know why some iron would chill and some would not chill while having the same composition. I made a pattern that was stepped down from 2″ to ¼″ by quarter-inches, in such a way that one end was a two-inch cube, and the other only a quarter-inch plate. In the melting of this iron in the open-hearth furnace, I took tests from the time the charge was first melted as it got hotter, and until the heat was ready to tap. I then took iron from the bottom of the bath as it was tapped and simultaneously dipped out a ladleful from the surface. It takes perhaps 30 to 45 minutes for a 12-ton heat to be run out into small ladles. The end of the heat is sometimes burnt or overheated, while the first part of the tap is sometimes cold. By taking the two tests as described above, I got what I considered was the hottest and the coldest iron at the same instant. I then broke the test castings, and found the thin ends all white; but when I broke them further back, the iron that was dipped out of the hot surface of the bath was perfectly white, while the iron taken from the first of the tap was perfectly gray. Thus you see the effect of temperature of pouring. One sample of the iron was probably 300 to 400° hotter than the other. The composition was about the same, but one sample had received the full heat along the top and the other had not. To overcome this difficulty of unevenly heated iron, I designed an open-hearth furnace that had three spouts. This enabled me to tap 5 tons from the top first, and get the hottest iron; then by tapping the second spout I got 5 tons more, and finally I tapped the bottom spout for the last 5 tons of a 15-ton heat, thus always getting good hot iron.

THOMAS D. WEST.—It is very important in making chill tests, to note the fluidity of the iron. With the same ladle of iron, in pouring two molds—the one with the iron fresh from the cupola and the other after the iron had been allowed to cool as low as possible for producing solid castings—I have found a quarter-inch difference in the depth of chill in 2½-inch square bars, showing unmistakably that the temperatures influence the results very greatly. Mr. West.

MR. WHITNEY.—In connection with Dr. Dudley's remarks, it is to be remembered that certain very small differences are very important, whereas others, as great numerically, are such as to offset each other as far as their general effect on physical character or on some of the physical qualities is concerned. In my car-wheel mixture practice a variation of as little as .03% silicon in the calculated charge must be compensated by other slight allowances, or the effect will be noticeable in a day or two in the daily test pieces. It may be of interest to show here again those two analyses discussed by Dr. Dudley in *The Iron Age* of February 29, 1886. At that time the differences (except in the case of sulphur) were not generally appreciated as very important. Mr. Whitney.

Good wheel required 150 blows of 25-lb. sledge to break it.
Poor wheel required 8 blows of 25-lb. sledge to break it.

| | Fe. | C. (total.) | Si. | Mn. | P. | S. | Graph. C. | Comb. C. |
|---|---|---|---|---|---|---|---|---|
| Good wheel ..... | 94.79 | 3.84 | .69 | .13 | .43 | .12 | 3.30 | .54 |
| Poor wheel ..... | 95.00 | 3.52 | .65 | .12 | .52 | .19 | 2.35 | 1.17 |

A calculation I made based on some work of Messrs. Carnot and Gontal and on metallographic principles indicates the following proximate composition and elements of structural composition:

| GOOD WHEEL. | | | POOR WHEEL. | | |
|---|---|---|---|---|---|
| MnS . .206 | | | MnS . .195 | | |
| FeS . . .121 | | | FeS . . .315 | | |
| FeSi . 2.045 | 5.127 | Impurities. | FeSi . 1.923 | 5.768 | Impurities. |
| $Fe_3P$ . 2.755 | | | $Fe_3P$ . 3.335 | | |
| $Fe_3C$ . 8.110 | 67.610 | Pearlite = | $Fe_3C$ . 10.130 | 84.492 | Pearlite. |
| Fe . . . 59.500 | 0.54 | Comb. C. | Fe . . . 74.362 | | |
| Fe . . . 23.963 | 23.963 | Ferrite. | $Fe_3C$ . 7.390 | 7.390 | Free Cementite. |
| C . . . . 3.300 | 3.300 | Graphite. | C . . . . 2.350 | 2.350 | Graphite. |
| 100.000 | 100.000 | | 100.000 | 100.000 | |

Mr. Whitney. The effect upon the structure is here shown to be enormous. Notice the moderate amount of ferrite, or material like wrought iron, in the good wheel and the total absence of it in the poor wheel, which has instead an excess of cementite. I assume that the original analysis was in each case from a heavy section of the plate of the wheel. While I know from practical experience with far smaller variations than this that enormous physical difference is directly due to such a variation, or kind of variation, as here illustrated, it is not yet clear to me how this is brought about; but I believe further study on the lines of metallography will show the exact application of the graphical charts of composition and temperatures so usefully applied in the case of steels and the purer irons.

Mr. West. MR. WEST.—My experiments have shown in every case that the hotter metal would chill deeper. There is only one proper way of determining such matters by practical tests, and that is, in remelting the iron to use the same cupola under exactly identical conditions. To accomplish this, I designed what is called a twin-shaped cupola. It is simply a round cupola with a partition down the center, and two slanting bottoms, so arranged that when the iron is melted on one side it can be run out without mixing in any way with the metal on the opposite side. By thus using the same fuel and the same blast, the charges in both compartments may be melted and maintained at the same temperature.

Mr. Voorhees. S. S. VOORHEES.—Is it not assumed that the carbon of iron in the melted state is combined, and that anything which will tend to lengthen the time that the metal continues in the melted state will give the graphitic carbon a better opportunity to crystallize out? If the metal is poured from a higher temperature, it will allow a longer time for this crystallization to take place. Silicon lowers the melting point; and high-silicon irons are gray, while low-silicon irons chill. Is it not possible that the raising of the solidifying point due to low silicon may shorten the length of time the iron is in the melted state, and thus not give the carbon an opportunity to assume the graphitic form?

Mr. Sauveur MR. SAUVEUR.—I think Mr. Voorhees' argument seems very probable. The formation of graphitic carbon depends primarily upon the length of the time taken by the metal to assume the

solid condition. It is while it passes from the molten to the solid state that graphitic carbon forms; and if the effect of silicon is to increase the length of this period, it must also increase the amount of graphitic carbon, and this we know it does. Mr Sauveur

MR. VOORHEES.—That seems to be borne out too by the experiment described by Mr. Moldenke in taking the metal from different parts of the bath, and other experiments too seem to bear that out. Mr. Voorhees

MR. MOLDENKE.—That is a point we know little about yet. The effect, as Mr. Voorhees rightly says, of a long-continued period of cooling will be to throw out more carbon; but the hotter the metal when poured, the longer that period, and yet the deeper the chill. This is exactly opposite to what we would expect to find. The iron cast from the top of the bath, as explained previously, was the hottest iron and took longest to cool, but it chilled the most. What causes this phenomenon we do not know yet. The white irons (or the low-silicon irons) melt much more quickly than the gray (or higher-silicon irons), so that another element is introduced of which we do not know much either. Mr. Moldenke

MR. SAUVEUR.—The formation of graphite does not begin until the metal begins to solidify; and it matters very little how long the metal was in the mold before it begins to solidify. In other words, it is quite independent of the casting temperature, as I understand the matter. It is during the transition period from the molten to the solid state that the graphitic carbon is supposed to form. It is quite possible that our knowledge on this subject is not definite enough; but I think that that is the general belief. Mr Sauveur

MR. MOLDENKE.—In spite of that, we do not know enough yet; because if the mold were cast with a very hot iron, one would imagine that the metal would merely stay fluid for a longer time before getting cold enough to chill; and that therefore the result would be the same, whether the metal be cast hot or cold, but that is not the case. Mr. Moldenke.

MR. WHITNEY.—That effect is more marked with the softer than with the harder irons. In the case of soft foundry iron, with a test such as Dr. Moldenke has suggested, using different Mr. Whitney

Mr. Whitney. sections, the smaller section will chill perhaps 3-16-inch in even very soft iron, when poured hot; but the same iron poured cold may not chill at all. Even if the difference is merely that a hand-ladle may be poured quickly at first, then held a moment and the upper half of the chill test poured more slowly and when the metal is somewhat cooler, the depth of chill will very rapidly decrease. The effect of initial temperature and of initial rate of cooling in the mold, is naturally greater as the silicon content increases, that is, in general, as the maximum capacity for chill decreases. Cold and slow pouring permits apparently a decomposition of some of the compounds in the iron to take place. High silicon favors such decomposition very evidently. The effect of other elements is more complex, but in practice these elements have constantly to be reckoned with.

# THE PRESENT STATUS OF TESTING CAST IRON.

By Richard Moldenke.

The following is summarized from conditions in America, as found in current specifications, in the technical literature, and through interchange of thought with the men identified with the testing of cast iron. Then, again through discussion with notable men in Europe engaged in similar lines of investigation.

Tests of cast iron are carried out in two ways: by means of a supposedly representative bar, and by testing to destruction. The latter way is the preferable one, provided that it is fair to both consumer and producer. Our car wheel and coupler tests come under this heading, and the testing of pipe will form the text of a special communication to us to-day by one of our members. Tests to destruction in other lines will follow as fast as the consumer is in position to demand them.

It is, however, universally recognized that this system of testing is applicable only to a limited range of castings, and that other methods must be adopted to assure us of good results for the general run of the foundry product. This has always been attempted by means of a test bar, with what chances of success remains to be seen. The tendencies were two-fold: First, the short coupon attached to the casting, into which coupon the iron might or might not enter under conditions identical with those of all parts of the casting. Usually the chances were against even a fair representation, and this form of testing is deservedly looked upon with less favor than formerly by those who understand something of the characteristics of cast iron. The second method was to use very long bars (often five feet) and test them transversely by the gradual application of a stated load. The bars gave results depending upon the manner and time taken in loading, and are now known to be altogether too favorable to the founder. Moreover, the conditions of the test were exactly opposite of those met with in practice. An impact test would have been more suitable.

We have, however, to-day exactly this style of bar in use in Germany. About three feet long, square, and of pretty small

cross-section, the bar is broken transversely, and tensile test specimens cut from the broken pieces. On the other hand, our German friends are desirous to have further light on the subject, and have appointed a commission to study the matter thoroughly. They further compliment us by utilizing our results in their investigations. The German specifications for pipe will no doubt be touched upon by another of our speakers to-day, for herein they have also been active in the Fatherland.

Many have been the tests made on bars, and each line of investigation has had the benefit of at least some previous recorded experience. The result has been the crystallization of ideas which has probably found as good an expression as any in the recent report of the Committee appointed by the American Foundrymen's Association, wherein short, heavy round test bars, and these cast on end, are recommended.

There is, however, another idea embodied in the report just alluded to, and this is so different from those generally accepted that it must be mentioned here. Our American founders, who know the material they are producing daily at least to some little extent, recognize the fact that even with the best of iron, a casting may be of inferior quality through improper conditions existing at the time of pouring, and this unknown to the average layman. They further know that a weak or unsuitable iron can be strengthened artificially, when making test bars, so that it may pass muster, and yet the castings will be inferior. For these reasons, therefore, in adopting the specifications they did, our American foundrymen cut loose from the very laudable but entirely impossible production of test bars which are held to be identical with the casting, and selected a method which would give as nearly a true valuation of the quality of the iron going into the casting as possible. If afterwards, a casting could be tested to destruction, so much the better. The fact that the iron which goes into a casting is found to be of a suitable quality, and of sufficient strength, is the only safeguard which can really be expected from the test bar anyhow. This presupposes that the iron is cast into bars under conditions as fair as possible in the light of our present knowledge; that the bars are of the size and shape which will neutralize as much as possible the variations due to the sand, the pouring temperature, and the pouring

method, and that no artificial strength or avoidable weakness be tolerated. By making such tests regularly, a founder can quickly satisfy himself, and possibly his customers, on the score of "quality" so far as the iron poured into castings is concerned. Even tests to destruction are not always as satisfying.

The present status of the question under discussion seems to indicate that the subject hinges upon whether producers will open their works to customers, and give them every facility to judge for themselves the quality of the iron made by means of the tests just spoken of; and on the other hand, whether customers will accept this as a guarantee of good faith, and the desire to give them the best work that can be made. The customer, it may be added, quickly becomes a competent judge of the good or bad conditions prevailing in the various foundries from which he draws his castings, when in contact and direct touch with them.

NOTE: The joint discussion of this paper and the succeeding one on "The Need of Foundry Experience for the Proper Inspection and Testing of Cast Iron," by Thomas D. West, follows on pp. 213-216.

# THE NEED OF FOUNDRY EXPERIENCE FOR THE PROPER INSPECTION AND TESTING OF CAST IRON.

By Thos. D. West.

Were the title of this paper of my own selection, I might have added also, "the need of foundry experience in formulating methods of testing," for, in the past formulation of methods, largely lies the origin of evils which show the necessity of foundry experience in judging of what is best or practicable in testing cast iron. The compilation of all methods now used for testing the physical properties of cast iron would result in such a variety as to bewilder one not broadly experienced in the different branches of founding. Had all the originators of methods now in use had a broad experience in founding, some of these methods would never have been proposed.

Of all metals cast iron is the most complex in its physical structure. This is because of the fact that changes in the rate of cooling like grades of iron and cross-sections produce radically different crystalline fracture or grain in the iron. Two pieces of the same cross-section and poured from the same ladle can, by a difference in the rate of cooling, be made entirely different in physical structure, one having a soft open grain, and the other a hard dense grain. Again, with uniform sections and a constant rate of cooling, but varying proportions of silicon, sulphur, manganese and phosphorus, we can produce different grades, varying in degrees from a very soft, open grained iron to a hard or chilled one.

Changing the grade of an iron by one or the other method just mentioned, is readily achieved by any one experienced in such work. The question may be asked, if the ability to do this or direct such operations would be of any value to an inspector or tester. Where inspection of appearances, as to smoothness and beauty of finish only is required, or where work is duplicated, as in the case of car wheels, and subjected to certain prescribed tests that have proved effective by numerous trials, lack of foundry experience on the part of the inspector may not

be of vital importance. Where the inspection of castings is, however, for determining the strength, solidity or durability of castings that cannot be tested by breaking a duplicate, it is rare that the possession of foundry experience does not prove of value, as the test bar or any other special method of testing may often fail utterly in indicating the true character of a casting. For example, when castings like rolls,' punches and shears, fly wheels, etc., break from usage and the fracture exhibits shrink holes or a very porous grain compared to other sections, wherein has the test bar or chemical analyses proved of value, in showing a strong iron or apparent solidity? In connection with the test bar and chemical analyses, a broad experience in founding will in almost all cases prove most valuable in passing judgment upon the durability of these castings. Foundry experience cannot give an inspector an X-ray eye for seeing the interior of castings, but it will, to a certain extent, enable him to anticipate the results by observing the methods used in pouring and feeding to supply the shrinkage of the parts that might be weak from porosity or shrink holes. There may be cases where an inspector experienced in broad founding can fortify his position by seeing a mold before it is closed, and being upon the ground at the time of pouring and feeding a casting. It is not uncommon to have a mold provided with proper feeding heads and pouring gates, and yet the feeding is such as not to produce a solid casting. This may be due to ignorance or carelessness on the part of the molder or to mishaps over which there is no control.

Aside from shrink holes, blow holes occur, and it requires foundry experience to judge of their existence. There are certain lines of castings that may have blow holes, while others have not. It generally requires some experience to distinguish a shrink hole from a blow hole, and one may be as injurious as the other. As, in the case of shrink holes, blow holes may often be best predicted by seeing the methods of molding and casting, and the broader the experience of the foundry inspector the better qualified he will be to pass judgment upon the solidity or strength of such castings.

Again, the inspector should be competent to judge the evils of contraction. When castings are of disproportionate sections, one part will contract more than another, giving rise to internal

stresses that no test bar records can properly reveal. Such stresses may be largely overcome by methods of cooling after a casting has been poured. Experience in founding will enable an inspector to determine at what points weakness is likely to exist, and where to look for slight cracks.

Sufficient has been outlined to illustrate that much is needed beside test bar records to pass upon the durability of a casting. Nevertheless, the test bar is often a very important element, and in designing the form and size and method of molding and casting test bars, a broad experience in founding is again essential.

The want of this experience in the past has resulted in the advocating of methods that have caused much contention and confusion of ideas, retarding the establishment of truly practical systems of testing.

This paper might be extended to considerable length in support of the claims put forth, but time being limited, generalities only are given to bring out discussion along lines which, if closely followed, cannot fail to prove that, while there is much need of broad foundry experience for the proper inspection and testing of cast iron, such need can be greatly reduced by improving the methods of testing, a work which comes directly within the scope of this Society.

# DISCUSSION.*

ALBERT SAUVEUR.—In regard to Mr. West's reference to the structure of cast iron as most complex, I wish to take exception to that qualification. We hear a great deal about the complexity of cast iron, but I think it must be admitted that cast iron may logically be considered as a mass of steel mixed with particles of graphite; so that the greater complexity of cast iron compared to steel does not exist in fact, at least not in my estimation. It is true, however, that cast iron has a very sensitive structure, a structure that is very easily altered, especially by the rate of cooling, and by its chemical composition. Mr. Sauveur

THOMAS D. WEST.—If a steel man can show anything more radical than an iron founder can in taking iron from the same ladle and producing one piece of metal entirely white and close-grained and another piece gray and very open-grained, I shall concede the point. Aside from the question of structure we have the shrinkage and contraction of cast iron to contend with. I make a distinction here between the terms shrinkage and contraction, because they are really two different factors, and we ought to have different terms to define them. Shrinkage is the change of volume of the metal in cooling from a liquid to a solid state. In making very massive castings like rolls, or punches, for instance, anybody experienced in that work knows that such castings must be fed by occasionally supplying additions of liquid metal or by providing self-feeding heads; otherwise shrink-holes will be created. This factor I define as shrinkage. After the metal has solidified, then in continuing to cool, its volume decreases. This I define as the contraction. These two elements are not only very troublesome, but often very expensive. It is on these and other grounds that I might mention that I base the statement that cast iron is a more complex metal than steel. Mr. West.

* Joint discussion of the two preceding papers, viz: "Present Status of Testing Cast Iron," by Richard Moldenke, and "The Need of Foundry Experience for the Proper Inspection and Testing of Cast Iron," by Thomas D. West.

Mr. Sauveur.

MR. SAUVEUR.—I think that Mr. West's argument to prove the existence of this complexity only shows the great sensitiveness of cast iron to heat treatment, a point which we are all prepared to admit.

Mr Outerbridge.

ALEX. E. OUTERBRIDGE, JR.—It may be interesting to recall an investigation made in 1888, which exhibited clearly the important influence of size of test bars upon the tensile and transverse strength of cast iron. In one experiment we cast a solid block about 15" x 14" x 14" of fairly strong iron. We sawed this into eight slabs and planed them all to a uniform thickness of one inch. We then cut these slabs into square bars 1" x 1" x 14", making altogether sixty-four test bars. The bars were numbered and broken on a transverse testing machine, with supports 12" apart. The highest records of strength were found in the outside rows, as might naturally have been expected; the average breaking stress of the bars forming the bottom row was about 2 800 lbs., the fracture of the metal being close-grained. The bars cut from the interior of the block were, of course, much weaker and the fracture showed a very coarse grain. The average breaking stress of the bars from the center of the block was about 1 750 lbs. This enormous difference of strength in different portions of the same casting, indicates how greatly cast iron differs from cast steel in this particular. In order to ascertain the effect of variations in size of test-bars, upon the tensile strength of cast iron a number of bars were poured from one ladle of iron, all the bars being of the same length, viz, 15", but of different sizes, ranging from 1½" to 2½" diameter; these bars were all turned to the same size, viz, 1.129" for a length of 10" between shoulders, the ends being threaded for the grips of the testing machine, and pulled on a testing machine having a capacity of 100 000 pounds. There was a gradual decrease in strength exhibited as the diameter of the castings increased; the difference in tensile strength between the 1½" bar and the next larger bar (which was cast 1 5-8" diameter) amounted to nearly 5 000 pounds. The tensile strength of the 1½" bar was about 33 000 lbs. per square inch, while the tensile strength of the 2½" bar was about 21 000 lbs. per square inch. These tests serve to show the importance of adopting standard sizes of test bars for determining the strength of cast iron when poured into castings of varying dimensions.

THE PRESIDENT.—Experience has shown that we cannot cast two test bars out of the same ladle that will give us the same results. We have apparently done everything that we can, and yet two test bars, identical in size, cast at the same time out of the same ladle and with the same runner, all connected together, will not give accordant results when tested. We began with bars 2 inches square, and we found differences of 1 500 to 2 000 pounds in the breaking load, applied centrally on a 12-inch span. Then we cast round bars and tried those in the rough, and they gave us somewhat higher differences. Then we cast the round bars larger and turned them up in a lathe so as to have exact diameters; but the results were still unsatisfactory. It has been said that we do not know anything positive about cast iron, and our experience with test-bars points that way. The President.

In a recent conversation with Professor Howe, he observed that this state of affairs was not at all surprising to him; that, since cast iron is a supersaturated metal, the total carbon is far above what it can contain in the combined condition, and infinitesimally small differences in the rate of cooling may work great differences in those characteristics which contribute to the strength.

Our latest struggle has been with the testing machine itself. In our machine we move the weight on the scale-beam by hand, to correspond with the increases in stress; and since cast iron has very little deflection, it often happens that the piece snaps before, or even during the movement of the weight on the scale-beam, giving, of course, unreliable results.

MR. WEST.—It is a difficult matter to cast two bars from the same pattern and get them of exactly the same size. A slight difference in the sectional area of the two test-bars may account for a difference of several hundred pounds in the strength. I think, however, if three or four bars were cast on end and at the same time, out of the same ladle, the average strength of the set will give a fair basis of comparison with another similar set of tests. Mr. West.

R. G. MOLDENKE.—In a bar tested transversely, surface cracks may cause premature rupture and a consequent loss of several hundred pounds in the breaking load of an ordinary test bar. Mr. Moldenke

The President. THE PRESIDENT.—We have always cast our bars on end. We tried the square bar first; then we tried the round bar, and we found that in cooling it became a little crooked. Then we made larger bars and turned them up; but since they were still a little crooked, consequently in the turning we took off a little more of the first iron that cooled on one side, than on the other, leaving a test bar with a surface composed of metal that had cooled at different rates, with consequent different strength in different parts of the surface. We cast bars 2½ inches in diameter and turned them down to 2 inches in the hope of eliminating this difficulty, but we have not yet succeeded in getting satisfactory agreement in results. Variations of over 10% in the results are common. Annealing tends to increase the uniformity, but decreases the strength.

Mr. Sauveur. MR. SAUVEUR.—The results of the very interesting experiments which Mr. Outerbridge has described all go to show the extreme sensitiveness of cast iron to the rate of cooling. They merely constitute additional evidence of a fact which has been repeatedly and conclusively demonstrated; they do not indicate the complexity of cast iron. The belief in the complexity of cast iron is, in my opinion, erroneous and mischievous, and its circulation should be strongly opposed.

# A QUICK AND AUTOMATIC TAPER-SCALE TEST.

## Proposed as a Standard Form of Contraction Test for any Cast Substance and of Chill Test for Cast Iron.

By Asa W. Whitney.

Iron foundry methods for the determination of contraction of cast metal from size of the mold are, so far as the writer's experience goes, sufficiently accurate except where bars of unsuitable cross-section are used or where the ends of the bars are cooled too quickly by being cast against the iron yoke from which contraction is measured. Such yoke should touch only a part of the faces of suitable small lugs so cast on the bar as not to interfere with the test for transverse strength.

Contraction is usually measured by a micrometer screw device set in one end of a yoke of exactly the same length as the one used in the test bar mold. It is also often measured by a graduated wedge or taper scale. The results are usually expressed in hundredths of an inch per foot of the test bar. I prefer to express it in percentage in connection with the metric system so convenient in physical testing. I think it is generally conceded that the cross-section of gray iron test bars for regular comparisons of transverse strength should in all cases be large enough to cool in the sand mold without chilling at all if possible.

I do not recommend a cross-section of less than 1½ square inches, even for soft iron, and in my own practice prefer a bar of hexagonal cross-section, and have used such as large as 4 square inches area.

In regulation of chilling mixtures I find it very useful to have bars of the same sectional area, but flat, so cast between chillers as to be totally white. The contraction of these solid white iron bars also varies according to composition, as does their strength and elasticity. Even these bars, however, do not cool quickly enough to be used within several hours as contraction tests, without interfering with their value otherwise as test bars.

To make a test for contraction of cast iron more rapid, the

phenomenon of chill is necessarily introduced, and it is best, therefore, that the conditions should be such that the maximum chill is obtained for the given composition, mass and temperature. That is, the initial contraction must cause no separation, or insulating air space, between the test piece and the chiller,* as this tends to reduce both the total contraction and the chill by a sudden alteration of the cooling conditions.

In the determination of chilling capacity of iron the usual methods are crude. The contraction causes variable time of contact or variable insulating space. In the rough chill tests made on the side of a sand molded pig at blast furnaces, the usual excessive heat of the pig and the scale on the chilling block are sources of further error. The Whitney Chill Cup, introduced into car wheel practice about 1872 and adopted at several blast furnaces, secured much greater accuracy as a means of test for chill over a considerable range of composition. The iron was poured from a hand ladle at a proper temperature into an oblong cup-shaped cast iron mold (about 6″ x 3″ x 1½″ deep) whose bottom was flat (except for a transverse ridge to facilitate subsequent breaking of the test piece) and whose sides were slightly flared. Uninterrupted contact with the bottom was thus attained. The chilling effect in hard iron, however, was complicated by the effect of the exposure of the top to the air and by cross chilling from the sides. Many experiments have proven the value of *maintaining* contact with chiller to obtain maximum chilling effect and the various forms of "contracting chills" for chilling cast iron wheels are made with this in view.

Both the variations of rate of contraction and those of chilling capacity encountered in the range of cast iron depend (other conditions given) upon the chemical composition; but any given change in the composition does not necessarily cause these phenomena to vary together in the same degree or even in the same direction Taking the range of cast iron, however, as a whole, the capacity for chill and the rate of contraction do in general vary in the same direction, whether owing to composition or to the rate of cooling.

* I prefer to use the term "chiller" rather than the foundry word "chill" which is used to designate both the iron cooling device and the effect of same on the metal *if the metal touching it is found to be white or "chilled."* The metal is chilled by such device even if its composition does not allow a "chill" to form under the conditions.

In a very valuable paper entitled "Contraction and Deformation of Iron Castings in Cooling from the Fluid to the Solid State" (*American Society of Mechanical Engineers*, December, 1896), Mr. Francis Schumann gives a formula by which the contraction of an iron casting may be calculated from that of a test bar of a different cross-section but of the same composition. In my paper entitled "Transverse Strength of Chilled Car Wheel Metal" (*Journal of the Franklin Institute*, April, 1897), I showed the advantage of chilling a test bar from opposite sides only. to develop the maximum strength, particularly when under such conditions the iron tested became wholly chilled or white. The strength in the direction of lines of chill being 90% to 100% greater than at right angles to that direction. I believe, though I have not definitely proved it, that the rate of contraction also, in such wholly chilled iron, is slightly greater in the direction of the more rapid dissipation of heat (that is, in the direction of the chill crystals) than in any other.

Early in December, 1900, when regulating air furnace mixtures, I devised and roughly tried a contraction test particularly for hard, easily-chilling cast iron, but because of other duties have had no opportunity as yet to fully develop it in practice. It was made to conform to the general principle of the "taper scale" by which caliper measurements, for instance, are denoted or registered by distances measured off vertically to the actual length calipered from side to side of the scale. By this means, using a suitable angle of taper, differences of caliper, otherwise recognized only by means of a vernier or micrometer screw, are read as easily visible spaces indicating definite micrometer measurements. With fairly uniform conditions of use and unyielding substances, the accuracy of this method is satisfactory.

My aim being to preserve, in spite of contraction of the cooling casting, its continuous contact with the chiller (for the sake of maximum chilling effect) and being also to indicate automatically, in a practical way, the progression and extent of contraction of a test piece under such standard cooling conditions, I naturally evolved the form of a frustrum of a cone for the test piece from the idea of the taper-scale.

The design and methods of molding the top and bottom of the test piece being dependent upon the sort of metal or other

substance cast, and other conditions, are not specifically described here as they may be devised by the founder in each case. As I have designed and adjusted it for my purposes, mostly hard or "chilling" mixtures of iron, the test piece *as cast* is a frustrum of a cone whose top diameter is 5.642 cm. This gives an area 25 sq. cm. (3.875 sq. ins.), or the same as that of the cross-section of the test bars I advocate (instead of 4 sq. ins. as heretofore), hexagonal in section for grey bar, and rectangular in section for chilled bar. I assume 5% as the maximum range of contraction to be possibly allowed for in any metal cast in chilling mold this size. Five per cent contraction of the diameter 5.642 cm. would reduce the diameter of the top of the frustrum to 5.360 cm. The bottom of the mold is therefore made of this latter diameter inside, while its top is 5.642 cm. diameter inside. By making the vertical height of the mold between these inside diameters just 10 cm., a linear contraction of .5% allows the top diameter of test piece to fall 1 cm. below its original position, and 5% contraction would just allow it to drop out at the bottom of the mold. The usual range for gray irons cast of this section and mass (mold being made of gas carbon or the like) would possibly vary from about .5% to 2%, and for totally chilled iron (mold being made of cast iron) from about 1.8% to 2.5%. 2.5% contraction of the top diameter would allow a vertical fall of 5 cm.

*Precaution in Casting.*—As soon as the test is poured any film of iron remaining in the sand pouring basin is promptly separated from the gate or the sand loosened to allow it to settle with the contraction of the test piece. As soon as this begins the sand is removed or allowed to drop out from below the movable iron or carbon bottom, which then drops and allows the contracting test piece to project. To hasten the cooling and to be sure no iron in the gate or vents interferes, remove the cope, and by a light rap in case of chilled iron break off the projecting gate. When black and sufficiently cool, the test piece may be further cooled by a blast of air followed by hot and then cold water. The mold at the same time should be cooled by a fine spray of water and blast of air. With an iron mold and hard iron, all this should be done in 10 to 12 minutes from the time mold was filled. An approximate measure of contraction could

be taken before removing test piece for cooling, but when cooled it should be dropped back into the fairly cool mold for close measurement. The rate of contraction of the top diameter is expressed by one-half the number of centimeters of vertical drop of that diameter or top of frustrum. The projection of the piece at the bottom of the mold would of course give slightly less than the correct rate, as it is slightly reduced by the vertical contraction. As this latter (the amount of vertical contraction) is due to the rate of contraction *across* the chill crystals, it would be interesting and convenient for purposes of study to compare it by *micrometer* measurements with the rate of contraction as determined from any diameter of the mold.

Such a test piece made in a gas carbon or other poor heat-conducting mold would be quite gray in fracture in most cases. These test pieces, whether chilled or gray, though short, could then be broken (after nicking and properly supporting the ends) by a sledge blow on a "hard edge," and one-half (or enough of the half to show from centre to circumference) be prepared for metallographic examination. This grinding, polishing, etching and observation need not take more than 15 to 20 minutes, whether the sample is wholly or partially gray or chilled.

Either half would be suitable as a sample for determination of specific gravity and for testing hardness by Professor Turner's sclerometer or other means, and for determination of strength by Grinnell's method of indentation by a hard steel ball of 10 mm. diameter and reference to tabulation or formulæ.

As to the accuracy of my suggested test of contraction, I believe it will be found satisfactory when its promptness and advantages as a chill test, etc., are considered. I should estimate the probable error as not *more* than plus or minus .2% of the assumed top diameter (or a total *range* in vertical measurements of 8 mm.). This is equal to plus or minus .024 inch per foot, while the usual contraction measurements on a 12″ length of bar have an error of not *more* than .1%, or say, .012 inch per foot. But if necessary in this cone test, the *exact* contraction also can be readily obtained by direct micrometer measurements of the test piece and of the chill mold in case the latter alters with use.

I advocate the use of a non-chilling mold such as gas carbon

or graphite mainly as a step in the process of arriving at the value of the chilling mold and its closer comparison with usual transverse or tensile test bars of the same cross-section. There are, of course, certain special precautions to be taken in making and treating the cast iron chilling molds before final turning to size in order that heat may have little effect. I believe that I can make them to stand the work and retain their dimensions sufficiently well for several hundred tests of cast iron, and longer for more fusible metals or other substances to which the test may apply. Of course for very soft iron and for other metals, a smaller size and a different taper may be better. It may be advisable to insert a sliding shutter to cut off the gate at base as soon as poured, before it can feed the contracting casting. I figure that as here designed, an iron which would show only ¼″ chill on A. Whitney & Sons' regular chill test block (1½″ x 2½″) cross-section chilled on 1½″ side would just chill in this test to the centre (=chill 1 1-8″ nearly), the "relative chilling power" of these tests being calculated as 39.8 to 184.6, or by metric measure, as 15.6 to 72.75. In other words, the normal factor for chill on the slower test is 21.5% of that on the new test. By Mr. Schumann's formula I figure that an iron showing .125″ per ft. (1.0416%) contraction in a 1-inch square bar will, at the mean proportional cross-section of this casting, if similarly made in *sand*, contract .9417% or .113-inch per ft., and similarly the top diameter would contract .9373%. Also when the top diameter contracts 2% the hexagon bar of same area of cross-section, if similarly chilled, would contract 2.0079%. But if cast as usual in sand, the contraction of such hexagon bar is known by experience to be about .15″ per ft., or 1.25%. The difference, say .75%, is due to the slower rate of cooling in sand. That is, a normal factor by which the contraction rate of the hexagon bar would be calculated is 62.5%. Examination of the micro-structures shows in similar cases the cause of the lesser contraction to be the greater decomposition of the cementite in the mass cooling the slower. That is, the $Fe_3C$ takes less room than the sum of its components when separated as ferrite and graphite; therefore in any iron, the *increase* in contraction in this test over the amount calculated from a casting in sand, is due to the more rapid rate of cooling which would be practically

a constant for the mold. A comparative study of the microstructure of the same iron cast in a gas carbon and in an iron mold would also reveal the kind and amount of change in structure to be allowed for in slower cooling, that is, it would at once connect the phenomenon of contraction and its relation to specific gravity, hardness, chill and strength with the phases of structure which the rate of cooling can impose upon a given chemical composition. The final practical result would be prediction and adjustment of composition to requirements, with a minimum of chemical and physical testing. A normal microstructure being observed, normal factors for chill and contraction in other sections would be used. Abnormal structure would indicate promptly the proper corrections.

Without going very accurately into the calculations, I find for instance, that a certain composition of iron which would chill about ⅞″ to 1″ deep in the regular block chill test above mentioned, and which would be, of course, *totally white* (cementite, pearlite, and possibly Martensite) in this cone test, would be totally white also in a bar of rectangular cross-section 3.75 x 6.67 cm. (25 sq. cm. area) and would be *totally grey* (pearlite, ferrite and graphite) in a hexagon section of same area cast in sand (whose side is 3.102 cm.). Its rate of contraction might be as low as .22 inch per ft., or 1.84% in the cone test. From this I roughly figure that the gain in specific gravity when solid and cold over that when molten is about 8.2%. The specific gravity of such chilled mass, say of car-wheel iron, would be about 7.6, which is therefore 108.2% of the specific gravity of the molten iron. The specific gravity of the very fluid molten iron would therefore apparently be about 7.03. This agrees with the fact that wholly chilled cold car-wheel iron sinks at once, but rises later in molten car-wheel iron, while a cold piece of soft gray iron floats at once and until melted in.

It appears to me that the use of this test and some calculations on the results, along with metallographic examination of the same cross-section under other known conditions of cooling, would very soon make it possible to predict from such a test more accurately and promptly than now, the proper change, if any, to be made on large masses of metal waiting in furnace or ladle for large castings, and also to give valuable and timely

indications of the allowable and of the improper sorts of changes for the next heat which may need prompt attention. Any analysis of the test piece or of a shot sample would be directed by the indications of this test. Such a case, for instance, as a high rate of contraction with an undue amount of graphite at the centre of the cross-section, lying in pearlite or ferrite according to class of mixture, would need prompt treatment, and not always analysis, first.

Knowing the calculated mixture and the oxidation and fluxing effects of the melting operation, an examination of the micro-structure should enable us to avoid much analysis of the casting. In fact, from the ultimate analysis, say deduced from the calculated charge of furnace, it is now possible, thanks to the work of Carnot & Goutal (*The Metallographist* Vol. IV, No. 4), to fairly estimate a proximate analysis and the possible total amounts of cementite and ferrite of a casting, and by the principles of metallography and the cooling conditions determine whether an excess of ferrite, cementite or graphite is likely, and about how much pearlite will remain, thus estimating the *structural* composition.

The amounts by volume of these structural elements are shown at once by the rapid processes of metallography. By assuming a specific gravity of, say 1.1, for the graphite, and say about 7.2 for the impure ferrite of ordinary cast iron, and say 7.3 for the impure pearlite, the relative areas of each, as observed, may be translated roughly into percentage by weight and checked by the knowledge of the calculated cupola or air furnace charge. Examination of analyzed samples at first is to be recommended.

I cannot yet show any samples cast in the style of test mold I suggest, or their photo-micrographs, but among a number of my photo-micrographs of known irons I have some which indicate in general what "chill" is and how much more fully metallography can define it than chemistry alone can. Photo. No. 1 shows the structure, pearlite, ferrite and graphite (660 diameters) of gray car-wheel iron at center of my former square bar of 4 sq. in. area (side 2″). Photo. No. 2 shows structure (97 diam.) of similar iron chilled, being center of tread of a light wheel. It consists of cementite (bright, unaf-

FIG. 1.

About 660 diam. Metal for standard chilled cast iron car wheels. Center of 2" sq. test bar. Contraction, 1.125%, modulus of rupture, 42 500 lbs.; tensile strength, 20,200 lbs.; resilience, 83 inch-pounds per pound of metal.

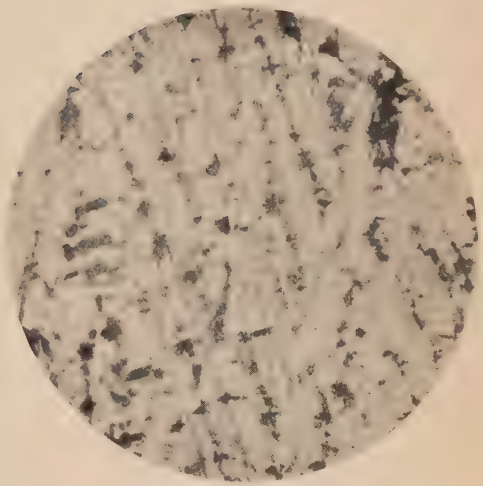


FIG. 2.

About 97 diam. Center of chilled tread of a 16" spoke wheel. The white or unetched areas are cementite (6.67% carbon) and the dark or etched areas are pearlite (0.80% carbon). Total carbon: about 3.74%.

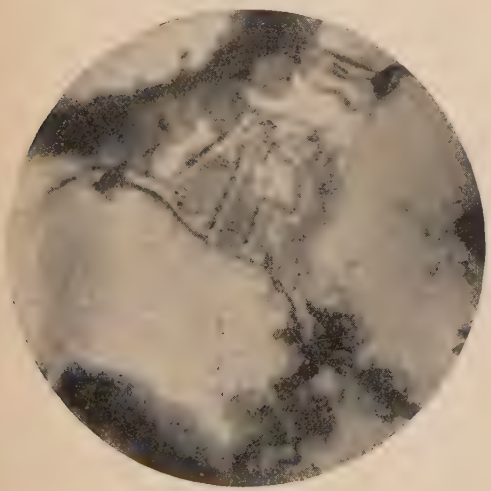


FIG. 3.

About 660 diam. Center of section of a very weak 48" diam. cast iron water pipe. Graphite and phosphide areas lying in ferrite. No pearlite, therefore no combined carbon.

FIG. 4.

About 97 diam. Center of section of a very good 40" diam. cast iron water pipe. Graphite well diffused and mainly in pearlite. Ferrite areas well distributed, as also the phosphide areas, which can hardly be found, and do not appear in the photograph at this magnification

fected by the etching acid) and of pearlite, which being lamellæ of ferrite and cementite, is affected and appears dark. Investigating by the ordinary methods of chemistry the difference between the gray and the chill of a chilled casting, we merely find that practically all the carbon is combined in the chill, while much or all of it may be graphitic in the gray. The point missed is, that for given composition and conditions, a certain amount of combined carbon goes to *strengthen*, and beyond that under proper cooling conditions all the rest goes to *harden*, or cause chill, while in the gray (slower cooled) it is the relative amounts of ferrite and pearlite, or the softening and strengthening factors, which ordinary analysis alone overlooks. But metallographic examination of a standard sample in connection with chemical knowledge of the charge indicates most promptly the general suitability of the composition and *what sort* of hardening or softening, if any, is advisable for the next heat, say to offset the effect of return scrap, or, by prompt use of my chill contraction test, what alteration can or should be made to a large mass of waiting metal. By comparison with some analyzed samples one can learn to estimate the amount of combined carbon, graphite, and even phosphorus, with valuable accuracy. Photo. No. 3 (660 diam.) shows the total absence of pearlite (steel of about .80% carbon), therefore of combined carbon, in a certain very bad 48″ water pipe. It also shows the hard masses of iron phosphide ($Fe_3P$) in areas of the phosphide eutechtic. These areas are like stones in the softer ferrite. Photo. No. 4 shows (97 diam.) a good 40″ water-pipe. Here, while there is even more graphite, there is no excess of phosphorus, and the graphite lies mainly in .80% carbon steel instead of in a weaker substance, ferrite. From the analysis of the latter sample, I have calculated approximately the micro-structure or volume of each structural constituent to compare with the photo. No. 4:

| | By Weight. | | By Volume (or area), as in Photo. |
|---|---|---|---|
| Impure Pearlite..... | 73.764% | = to about | 63.95% |
| " Ferrite ..... | 23.415% | = " " | 19.80% |
| Graphite found by analysis .. | 2.865% | = " " | 16.25% |
| | 100.000 | | 100.00 |

By means of a pocket slide rule such calculations are greatly facilitated. Probably within the limits of ultimate composition for any one kind of iron castings, the data given by systematic work with my contraction test sample would allow of tabulation of the usual ratios of graphite to total carbon for each rate of contraction and percentage of iron.

Of course the range of this device as a chill test depends upon the size of sample, which must be limited. For all mixtures softer than very heavy machinery castings a smaller size than I have shown may be better. It has always been my practice to test even the softest mixtures by either the regular wheel iron chill test or by using the mold for chilled ingots 1″ square by 4″ long, introduced some years ago by Mr. Outerbridge. In this connection it may be well to remark that just as there is no sharp demarcation between steel and white cast iron, so there is apparently no very definite distinction, as yet proved, between some of the hardening phenomena of steel and the chilling phenomena of very low carbon cast iron. But while in steel an allotropic modification of the iron evidently is concerned, it does not as yet seem necessary to assume that this is the case to explain the hardness of "chill" in cast iron. Chill seems to depend upon the total and relative amounts of cementite and pearlite which from a given composition can be segregated, fixed, or in other words, whose decomposition into pearlite and graphite or pearlite, ferrite and graphite can be prevented by either local or general cooling conditions. If the quotient of $\frac{\text{Perimeter}}{\text{Area}}$ of the cross-section of cast iron becomes large enough any ordinary composition of cast iron will "chill." The terms "chilling" and "non-chilling" as applied to cast iron are therefore merely relative. When we get down to the eutechtic of the iron carbon series (.80 carbon steel) I think we may say the phenomena of chill ceases, as there is no possible excess of cementite, but between .80% carbon and, say 2.5% carbon, the chilling of molten metal is little practiced, and it would require further metallographic research to cover this field and show the relations of "chill," if any, to the hardening accomplished by other processes.

I regard the following cases as particularly desirable to study

by metallographic examination of such a standard chill and contraction test as I propose.

Irons which would be white in a far larger section even if cast in sand. Irons which would show no chill whatever in this test. Irons which chill slightly, but show a dark graphitic line between the chill and the chiller. Irons which chill heavily in the centre of such a test and not at all next to the chiller. I am not sure that the latter phenomena can occur except in a sand, carbon or other poor heat-conducting mold.

In spite of the discussions and work (particularly the extensive series of tests by the American Foundrymen's Association), the general significance of the value of test samples of any kind seems to be even yet misunderstood by some; for instance, *The Engineering Magazine* for May, 1902, contains an article entitled "The False Witness of the Test Bar," by a British writer, Robert Buchanan. The test bar can tell the truth only directly in regard to itself. But by *knowing its relations* to other sections and adjusting the conditions of test and foundry practice, the discrepancies between its characters and that of other bars or castings can be properly allowed for. At least *some* progressive founders appreciate this. The wasteful and usually impossible practice of "testing to destruction" sample castings assumed as the only practical safeguard by Mr. Buchanan and some others is really only defensible as a preliminary experiment to establish certain relations with test pieces. A basis of "area for area" comparison of strength, referred to for bars of different sections, further vitiates his deductions. The relative strength of his 2″ x 1″ and 2″ x ¼″ bars can only be fairly compared by allowing for the relative strength due to form alone by means of their respective factors for modulus of rupture.

To show the necessity of knowing the relations to conditions and sections in order to properly use test bars, even if identical with the castings, the structural effect due to more rapid cooling in a particular direction must also sometimes be considered, as already referred to in regard to my chilled iron test bars. The modulus of rupture, even of a *slightly* hard bar, tested flatwise, is likely to be *greater* than that of a duplicate bar tested edgewise. Only by some reference to the *structure* can these cases be allowed for. With properly *related test pieces* and study on them by

metallography as well as chemistry, the effect of the *volumes* and forms of graphite and other structural units can be ascertained by routine of daily testing, if supplemented by study and calculations. This soon becomes memorized as experience, and should also be tabulated.*

The sort of work reported in that most valuable quarterly, *The Metallographist* and which now to most founders would appear as impractical as once did foundry chemistry, is the key to correct metallurgical practice, even in testing, as well in the foundry as in the many steel and other metal works where its value has been demonstrated.

* I regard this cone test as very suitable also for a study of critical points by pyrometric methods applied in casting or in subsequent laboratory reheatings or coolings in the easily regulated electric coil furnace The rate of fall of voltage of the thermo-electric current between test piece and mold in connection with critical points may be suggestive also. I have found this current quite marked and naturally in opposition to the direction of dissipation of heat. Some tentative work of mine indicates also the value of a standard sample such as this for electro-chemical comparisons of irons.

# STRENGTH OF WHITE IRON CASTINGS AS INFLUENCED BY HEAT TREATMENT.

By Alex. E. Outerbridge, Jr

In the year 1882, while engaged in metallurgical work at the car-wheel foundry of A. Whitney & Sons in Philadelphia, my attention was called by the inspector of wheels to an unusual and remarkable change that had occurred in the chilled or white iron forming the "treads" of a number of wheels poured from one heat; this change was first observed on removing the wheels from the annealing ovens. It was customary for the inspector to prove the hardness of the chilled treads by testing them with a cold chisel all around the "throat," or place where the tread joins the flange. On this occasion he found a number of wheels which were quite soft on one portion of the rim, extending the entire width of the tread for a length of six or seven inches, while on either side of the soft spots the chilled tread was so hard that the steel chisel slipped over the surface without biting. In order to study the nature of this singular occurrence I caused the wheels to be broken through the soft spots, so as to examine the fracture, and I found that the white iron (chilled iron) had been changed into perfectly gray iron, evidently after the wheels had been cast. The change was not equally well marked in all of the wheels and the soft spots were smaller in area in some cases than in others, but in all, the dividing line between the white portion and the gray portion of the chilled tread was sharply defined.

It is perhaps necessary to state that in the establishment where these wheels were cast it was customary to preheat the annealing pits by means of soft coal fires before the wheels were lowered into them, the flames passing through the pits or ovens. The rule was to close the dampers just before the pits were opened to receive the red hot wheels, in order to shut out the flames.

After careful investigation, I found that through an oversight the dampers of the annealing pits had not been closed and the flames from the fires impinged upon the surfaces of three or

four red hot wheels in the lower part of each pit, causing a complete change of the carbon from the combined form to the free condition wherever the flames touched the castings.

Drillings were taken for analysis from the soft parts of the chilled treads and also from other parts of the wheels, as well as from test pieces poured from the same ladles of iron. The analyses showed two things: First, that the car-wheel iron was of normal composition; second, that the only change in the metal of the annealed or soft portion of the "chill" was in the condition of the carbon, which had been converted from chemical combination with the iron into an amorphous form of graphitic carbon, or, to speak more guardedly, I would prefer to say simply into the form of "free" carbon, for there are reasons for believing that the carbon in this case is not in the same condition as when it exists normally in gray iron.*

Specific gravity tests showed that the gray cast iron resulting from this accidental heat treatment of white iron differed materially in density from the normal gray iron forming the unchilled parts of the same castings; the specific gravity was about 7.80 as compared with specific gravity of about 7.20 for the normal gray metal; a cubic foot of the gray iron produced by this annealing process therefore weighed about 37.5 lbs. more than a cubic foot of the normal gray iron of the same casting. It was noticed that the fracture was of much finer grain than normal gray iron, and "chips" or drillings of the annealed chilled iron differed greatly in appearance, size and shape, from the chips or drillings of the normal gray iron made with the same drill.

Several metallurgists to whom the pieces of annealed chilled iron were exhibited offered a plausible explanation of the phenomenon, saying that it was merely an accidental conversion of the white iron into malleable iron, and therefore presented no

* "Through purely chemical methods four conditions of carbon have been detected:
"1. Graphite or free crystallized carbon, not acted on by acids even when concentrated.
"2. Graphitic carbon or 'temper-carbon,' which is also in a free state and unaffected by acids, but presenting an amorphous appearance.
"3. The carbon of the carbide of iron (cement carbon) forming a definite compound acted upon by concentrated acids but insoluble in diluted cold acids. This carbide answers to the formula $Fe_3C$.
"4. Hardening Carbon, which appears to form a solid solution with the iron (or it is perhaps present as a compound dissolved in the iron) and which is acted upon by cold, dilute acids."
Notes on the Chemical Constitution of Cast Iron and Steel. By A. Carnot and E. Goutal. Annals des Mines, October, 1900

novel features. The analyses quickly showed the fallacy of this theory, for the total carbon was the same after annealing as before annealing, being about 3½% in each case, while in the ordinary conversion of white iron castings into malleable iron, a large part of the carbon is removed by the oxidizing material in which the castings are imbedded when subjected to heat treatment, and this conversion of white iron into malleable iron can only be successfully accomplished on sections of metal of moderate thickness, say less than three inches.

Although this accidental discovery of annealing of white iron on the treads of car wheels was regarded as an interesting and novel one at the time, the only practical use made of it was to guard against a repetition of the accident in the annealing pits of the car-wheel works with which I was at that time connected.

After the lapse of several years I was called upon to investigate in a professional capacity, and in the interest of some prospective investors, the merits of a new process of converting white iron castings into steel, and received from the manufacturer a number of axes, hatchets and other edge tools, which had been cast in white iron and subsequently converted into a metal having evidently many of the qualities of steel.

Without describing in detail the process to which these white iron castings had been subjected, I may say, briefly, that they were placed in the muffle of a furnace together with a chemical compound claimed to be necessary to the conversion of the cast iron into steel. Common salt and crude hydrochloric acid were two ingredients of this compound. The similarity between this process and the accidental over-annealing of the car wheels (with consequent change of the condition of the carbon) suggested to my mind that the chemical compound was probably unnecessary and that the secret consisted solely in the heat treatment, and I so advised my clients.

The conversion of white iron castings into dense gray iron having high tensile strength (approximating that of certain grades of steel) capable of being hardened and of taking a sharp cutting edge, is no longer a secret, and is carried out on a commercial scale in a number of establishments. While I cannot speak positively, I am of the opinion that the heat treatment

alone is now relied upon to produce the desired results. These products are not steel castings, though sold in some instances under the name of steel; neither are they malleable iron, but they may be described as occupying a peculiar position midway between cast steel and cast iron.

Having given a resumé of the history of this process of heat treatment of white iron castings, I will state a few facts regarding certain interesting features, including tensile strength, of this converted metal. In the early experiments I found that there was a vast difference in white irons, some samples remained white and hard after having been soaked for many days in the annealing furnace, even when subjected to the highest temperature below the melting point of the metal, while other specimens of white iron of the same dimensions yielded readily to the heat treatment and became completely converted into gray iron in a few hours at a comparatively low temperature. Analyses of the different samples revealed the chief cause of this difference. It depends almost entirely upon the presence or absence of silicon in the white iron; castings containing only a trace (or a few tenths of one per cent) of silicon cannot be successfully treated, for only a partial change of the combined carbon into free carbon takes place, even after prolonged treatment at the highest temperatures. Thus, in one of my tests, two white iron bars about 2 inches in diameter and 12 inches long, were placed side by side in the furnace and subjected to the heat treatment for eight hours. One of the bars, containing about 0.15% silicon, was cast in sand; the other, containing 1.25% silicon, was cast in a heavy iron mold, because if cast in sand it would not have been white iron. Both bars were equally brittle, equally hard, and perfectly white before treatment; a slight tap with a hand hammer was sufficient to break them. When removed from the furnace after eight hours soaking at a high heat, and allowed to cool in the air, the bar containing 0.15% silicon was unchanged in the appearance of the fracture, while the bar containing 1.25% silicon was almost gray; it was soft and so ductile that a small piece of it was flattened out under the steam hammer while cold. The bars were then returned to the furnace, subjected to the same heat treatment for six hours longer and allowed to cool, as before. No visible change occurred in the low silicon bar, while

the other bar was completely converted into gray iron with a uniformly fine grained fracture and dark gray color. This bar was turned in a lathe to a diameter of 1.129 inches (giving an area of 1 inch) and provided with threaded ends for the grips of the testing machine. The metal was readily machinable and the surface was free from "graphite pits," so characteristic of ordinary soft gray iron when machined. This bar was pulled on a hydraulic Testing Machine of 100 000 lbs. capacity, and the actual load at failure was 47 760 lbs. The elongation and reduction of area were not measured, but were very small.

A number of similar tests were made on bars of different sizes, giving tensile strengths ranging between 40 000 lbs. and 50 000 lbs. per square inch, but manufacturers equipped for making converted castings in this way have obtained much higher records than these.

The literature upon this novel and interesting subject is very limited, but some valuable papers have been published, and it is my desire to present herewith a resumé of the work done by several well-known metallurgists in this country and in Europe.

At the meeting of the Mining and Metallurgical Section of the Franklin Institute, held June 9, 1897, Mr. Charles James described "A Special Process for Treating Cast Iron," the following account of which appeared in the *Journal* of the Institute of July, 1897:

"White iron with approximately 2.40% combined carbon and 0.4% per cent graphitic carbon, is used for the purpose. The castings are placed in a muffle furnace, in which they are subjected to the action of a secret composition (said to be a powerful volatile oxidizing agent) and maintained at a temperature slightly below fusion for five or six hours. The product may be forged and tempered and shows a remarkable increase in tensile strength. The total carbon in the product seems to be the same as in the untreated iron, but combined carbon has been substantially altered into a finely disseminated uncombined carbon.

"Doubts were expressed in the discussion which followed, whether the result was not due entirely to the annealing rather than to the 'medicine.' The prolonged annealing was thought

to be a sufficient explanation of the facts. The process is in commercial operation in a large foundry in Philadelphia producing hatchets, hammers, etc."

The *Journal of the Franklin Institute* for September, 1900, contains an article by Mr. James on the same subject, giving abstracts of results obtained from daily business practice, extending over a considerable period of time, but though these investigations were made for the guidance of commercial operations they seem to be of sufficient metallurgical interest to justify their presentation.

Mr. James states that the irons used were all of Bessemer quality. The melting charges consisted of a mixture of gray and white irons, of which the following is an average analysis:

| | C.C. | G.C. | Si. | Mn. | S. | P. |
|---|---|---|---|---|---|---|
| White iron | 3.50 | .50 | .50 | .20 | .08 | .08 |
| Gray iron | .50 | 3.50 | 1.30 | .30 | .02 | .03 |

"The composition of the charges was regulated by the silicon content, which was made to vary from 1.20% to .90 of 1%, the average chemical composition of these mixtures being:

| | |
|---|---|
| Carbon | 3.40 to 3.80 per cent. |
| Silicon | .90 to 1.20 " |
| Manganese | .35 to .20 " |
| Sulphur | .05 to .04 " |
| Phosphorus | .04 to .03 " |

"The iron was sometimes smelted in a cupola, but generally in an air furnace. The following is the average chemical composition of a large number of castings:

C.C., 3.02; G.C., .47; Si., .78; Mn., .12; S., .05; P., .04.

"The weight of the castings ranged from half an ounce to 2 000 lbs. and over. The time required to effect the carbon change varied from 3½ to 10 hours from the time the castings had attained their full temperature.

"The temperature at which the change takes place in castings of this description lies between the melting point of silver and copper, and may therefore be approximately taken at 1.850° F.

"The change of carbon in castings subjected to the annealing process, though gradual, is co-extensive throughout any given

cross-section of the casting; no hard centre or core of white iron, surrounded by softer metal, having ever been observed in any of the castings examined. No matter at what period the annealing may have been arrested, the total surface of any fracture always showed a similarity in the condition of the metal, provided the composition of the metal was homogeneous and the heat treatment had been applied equally to all parts of the casting."

This agrees perfectly with my own observations, and it constitutes, moreover, a very interesting and important feature, differing from malleable iron castings where a progressive change occurs. The central portion of a malleable iron casting is sometimes found to be practically unaffected by the heat treatment as to the total carbon, and unless the casting is very thin, the conversion is, in fact, never uniform throughout the section. The same law of progressive change in the proportion of carbon obtains in "case-hardening" of steel, though the operation is the reverse, *i. e.*, carbon is imparted to, instead of withdrawn from, the metal.

The following analyses before and after annealing, given by Mr. James, show very clearly that the only change effected by the heat treatment is in the condition of the carbon:

| | C.C. | G.C. | Si. | Mn. | S. | P. |
|---|---|---|---|---|---|---|
| Before annealing ..... | 2.60 | .72 | .71 | .110 | .045 | .039 |
| After annealing ..... | .82 | 2.75 | .73 | .108 | .040 | .039 |

Mr. James says: "I wish, however, to say that, although for convenience the term graphite is employed in stating the analyses of these castings after annealing, the condition of the carbon differs very materially from graphite, either as found free in nature or as solidified out from gray iron during cooling. The carbon thus formed is evidently identical with what Ledebur has called 'tempering graphite carbon,' and is an allotropic form of graphite, and not merely amorphous carbon."

Mr. James states that both silicon and manganese exert great influence upon the carbon during the annealing process, "the presence of silicon being a necessary condition to the carbon change. In low silicon iron it is very difficult, and, in some cases, impossible, to effect the carbon change, no matter how long the iron is exposed to the heat treatment."

My original investigations did not indicate that manganese possessed any very marked influence on the carbon change in this process, though I am well aware that manganese, especially in the form of ferro-manganese, added in very small quantity to molten car-wheel iron, exerts a marked effect upon the chill, and on the relative proportion of combined and graphitic carbon. It is a fact, however, that low silicon irons are usually low in manganese, and this, I think, is the reason why Mr. James was led to infer that manganese facilitates the carbon change in the annealing process.

Mr. James gives no records of strength of the white iron castings converted by him into gray iron by annealing.

Before proceeding to the consideration of other features, I desire to call attention in passing to one point which may, I think, have practical value, especially in the manufacture of chilled cast iron car wheels.

It has long been known that wheels having approximately the same depth of chill on their treads wear very differently in service; some retain their hard surface until the white iron tread is gradually worn away evenly all around the wheel; others become soft in spots very quickly, necessitating removal from service long before the wheel has lived its proper life.

We know that the action of the brakes on the wheel heats the tread, and it seems to me not unlikely, from the foregoing observations, that when the silicon exceeds a certain amount, say 0.7% in any wheel, a similar kind of annealing of the white iron tread of the wheel may occur from the heating action of the brakes, thus changing the condition of the carbon, at least to a partial extent, and causing the chilled tread to become much softer than would happen under similar conditions, where the wheel is made of metal containing less silicon.

The most recent investigations bearing upon the subject under discussion corroborate the prior observations fully, and may be found in the *Bulletin de la Société d'Encouragement pour l'Industrie Nationale*, Paris, March 31, 1902, under the title, "Sur l'Equilibre des Systemes Fer Carbone," by G. Charpy and L. Grenet.

The authors call attention to the investigations of M. Bakhuis Roozeboom on an interpretation of known facts relating

to the constitution of metals formed of iron and carbon, published in the *Bulletin de la Société d'Encouragement*. November, 1900, and also to the investigations of Hugh P. Tiemann on "The Temperature Limits for the Separation of Graphite from Martensite in Pure Cast Iron," published in *The Metallographist*, October, 1901.

The authors say: "One can believe, then, as appears to be admitted by Mr. Tiemann, that the abundant separation of graphite, observed by Mr. Royston, was due to the presence of silicon in the white iron castings employed."

We have made a great many experiments on this point, and we direct attention in particular to the results obtained on five castings containing practically the same percentage of total carbon and in which the other elements are found only in small proportions, except in silicon, which exists in variable quantities. The following table gives the composition of these specimens:

| No. | Carbon. | Silicon. | Manganese. | Sulphur | Phosphorus. |
|---|---|---|---|---|---|
| 1..... | 3.60 | 0.07 | 0.03 | 0.01 | trace |
| 2..... | 3.40 | 0.27 | trace | 0.02 | 0.02 |
| 3..... | 3.25 | 0.80 | trace | 0.02 | 0.03 |
| 4..... | 3.20 | 1.25 | 0.12 | 0.01 | 0.01 |
| 5..... | 3.30 | 2.10 | 0.12 | 0.02 | 0.01 |

These specimens had been poured into cold water and did not contain any appreciable proportion of graphite, except the last, where we found 0.20%. Fragments of these specimens were subjected to annealing, more or less prolonged, at different temperatures. Six tables are given showing the length of time of each treatment, the exact temperatures, and the proportionate change of combined carbon into graphite for each specimen.

The following conclusions are reached; they are announced didactically as laws, and may be so accepted:

1. The separation of graphite commences at a lower temperature as the percentage of silicon is increased in the iron.

2. The separation of graphite having begun, it continues at a lower temperature than that required to commence the reaction.

3. At a constant temperature the separation of graphite proceeds at a lower speed according as the temperature is lower and the proportion of silicon is less.

4. The proportion of combined carbon which corresponds to

the equilibrium at a given temperature diminishes when the silicon is increased.

The brochure of Messrs. Charpy and Grenet is embellished with several fine micro-photographs showing very clearly the micro structure both before and after treatment. No records are given of tensile strength of the treated specimens and, as it is stated at the beginning of the paper that the white iron specimens were obtained by pouring the metal into cold water, tensile tests could not be made, for the samples were presumably merely small irregular lumps, which are far inferior, I think, for such purpose, to the uniform ingots of white iron obtained by pouring high chilling metal into heavy iron molds. I have found it easy, in this way, to obtain ingots of white iron of any desired length and of suitable sections, the dimensions depending, of course, upon the proportion of silicon contained in the iron poured. By using a flat ingot mold made of thick cast iron plates, I have obtained flat ingots 5″ x 8″ x ½″ of perfectly white iron and containing as much as 2% of silicon. These high silicon white iron ingots or plates are very readily converted into gray iron by annealing.

Although I consider it a misnomer to call castings such as hatchets, axes, etc., made from white iron changed to gray iron by heat treatment and subsequently hardened on cutting edges, steel castings, they are certainly very different from true malleable iron castings or from ordinary gray iron castings, possessing qualities more closely resembling steel.

When the process is carefully and intelligently conducted the product is reliable, but prejudice has been developed by the fact that some of the manufacturers have not thoroughly understood the underlying principles and have turned out castings imperfectly converted. It is, of course, much easier to make perfectly white iron castings from metal very low in silicon, but as I have here shown, such metal is unsuitable for the subsequent conversion by heat treatment into gray iron, and this I believe to be the main cause of many failures.

So far as I know there are no valid patents covering this process of heat treatment of white iron castings for conversion into dense gray iron of high tensile strength, capable of being forged and tempered, by simply changing the condition of carbon without removing any of the original component elements.

As the process of converting white iron castings into malleable iron is well understood and has been carried out on a large scale for many years, I have not thought it necessary to allude to this product further than to show that it is radically different from the converted white iron castings containing all of the carbon of the original metal before the heat treatment. Good malleable iron castings are very ductile and may be bent almost double, and twisted, while cold, without breaking, if the section is light enough to permit of complete malleableizing, but annealed white castings cannot be thus bent even when completely converted into gray iron.

It is true that since the discovery of the fact that hard and brittle white iron castings may be changed by simple heat treatment into strong soft gray iron castings, some makers of malleable castings modify their old methods and practically rely upon heat treatment to anneal their castings without packing them in oxidizing material as formerly, to remove the carbon, but such heat treatment does not and cannot make true malleable iron castings. In other respects, also, converted white iron castings, containing all of the original carbon, differ in physical properties from malleable iron castings in which a large portion of the carbon has been removed.

In conclusion, I would say that if we can eliminate the false name of "steel" which has been given to converted white iron castings, a distinct advantage will have been gained, for I believe that the new metal is worthy of taking a special place in the metallurgical arts, and I anticipate extended practical applications of the process as knowledge of the proper methods of heat treatment and of the valuable properties (as well as the equally marked limitations) of the metal shall become better known and appreciated.

## DISCUSSION.

Mr. Sauveur. ALBERT SAUVEUR.—I was greatly interested in Mr. Outerbridge's paper. I do not see clearly, however, why there should be such a sharp line of demarcation between the ordinary process of making malleable castings and the new processes of which Mr. Outerbridge has spoken. What occurs in a white cast iron when it is heated to a high temperature for a long time is quite well understood. We know that the combined carbon passes to a graphitic or "temper" condition—if that is the proper name; we also know that this passage from the combined to the graphitic or free state is helped very much by the presence of silicon. This is, after all, all that takes place in these so-called new processes. In the ordinary method of producing malleable castings, as I understand it, the casting is embedded in some oxidizing material which burns some of the carbon. The oxidizing substance coming in contact with the white cast iron, burns from the outside a certain amount of carbon, and the carbon migrates from the center to the outside of the piece, just as salt would dissolve in water; and the longer the annealing is continued, the more carbon is removed. The result is that in malleable castings, as generally produced, the center contains more graphitic or free carbon than the outside. The outside is necessarily somewhat decarbonized, but with this restriction I do not see any very great difference between these methods, and I do not see why the result should be very different. I can see very well that in these new methods little if any of the carbon is removed, especially if no decarburizing or oxidizing substances be used to surround the castings; but after all, I look upon these in the light of some very slight alteration of the ordinary method of making malleable castings.

Mr. Moldenke. R. G. MOLDENKE.—Did Mr. Outerbridge say that there are castings which differ from "malleable" castings, for the reason that nothing at all is removed from the casting?

MR. OUTERBRIDGE.—I did not intend to say that there was absolutely no oxidation of surface; probably there is, but the experiments that have been made by all those who have worked in the matter, so far as I know, show that the removal of carbon in this process where the castings are heated without being packed in any oxidizing element at all is practically *nil*. Mr. Outerbridge.

MR. MOLDENKE.—That is what I wished to find out. Now, if 1-16-inch be taken off of the surface of a piece of malleable casting, it will be found to be practically wrought iron, as far as carbon is concerned; and then if another 1-16-inch be taken off, the carbon will be .50%; the third 1-16-inch will show 1.50% carbon. When ¼-inch has been removed, there will be about 4.0% of carbon, or practically the full amount of carbon in the original casting. If this high carbon core be cut out and tested, it will give a tensile strength of probably 38 000 to 40 000 lbs. per square inch. In other words, the first operation in malleable castings is a change in the carbon, and the second operation is the removal of the carbon in the skin only. I can therefore see no difference between the method described by Mr. Outerbridge and the ordinary method of producing malleable castings, if we admit that the change in the carbon is a part of the malleable casting process. And this is the case, as it is well known that the change of the carbon belongs to the process. I make personally many castings which are so thin that we do not dare to put them in the pots with scale; but we have to coat them with fire-clay, and the oxidation of the carbon cannot take place. If a casting an inch square or two inches square be coated with fire-clay and annealed, the result will not be as good as where the carbon in the skin is given an opportunity to be oxidized; but there is really no reason why there should be a new class of castings brought out on this evidence, which are really only inferior "malleables." Mr. Moldenke.

STANLEY G. FLAGG, JR.—I should like to call attention to some information recently given me which seems to rather reflect on both conclusions. Some time ago the particular process described by Mr. Outerbridge was under discussion, and I called attention to the fact that the surface castings made by that process were very highly oxidized. The reply was that the Mr. Flagg, Jr

Mr. Flagg, Jr. process had been very much improved, that the castings were now all annealed in roll-scale. I then ventured the remark that I thought the nearer this process approached that used for malleable iron the better the castings became.

Now, on the other side of the question: in the western part of the country there are manufacturers of malleable castings who do their annealing without the use of any compound whatever. It is well known that malleable castings may be annealed in brick-dust, in lime and in various other compounds, but in the particular works referred to they use no compounds whatever. I have recently done some experimenting along these lines, and have made pretty good castings, but in the ordinary way of annealing it is almost impossible to prevent oxidation. The best way to prevent it is to use oil instead of coal or coke.

# NOTES ON CURRENT SPECIFICATIONS FOR CAST-IRON PIPE.

By Walter Wood.

I had expected to present to the Society at this meeting what might have been considered a complete "Digest of Current Specifications of Cast-Iron Pipe," because the subject has been under discussion between a committee representing the water works engineers of the country and a committee of manufacturers of cast-iron pipe. These committees, however, have not yet completed their labors, and what I am about to say will, therefore, only indicate the results which we hope to reach. The work is being taken in hand very earnestly, because every one desires to have a uniform plan of specifications. It may not be amiss to say that steps have been taken to have these specifications adopted, if possible, by foreign manufacturers, and thus secure international specifications for cast-iron pipe. The differences that exist in specifications for cast-iron pipe are not large, nor many; they have fortunately been kept within narrow limits, because the specifications originally drawn up by Mr. Kirkwood forty years ago for the city of Brooklyn have been the foundation on which all cast-iron pipe specifications have been built. The hardest point to overcome will be the impression which engineers have that good politicians will make good inspectors. There has been a great deal of controversy on that point.

I shall pass by those points of the specifications that a body like this, whose thoughts dwell upon testing, do not desire to take up, yet in doing so, I desire to bring out one thought, which is exceedingly important both for the manufacturer and to the buyer of material, namely, that a variation in weight of individual pieces should be adopted that will be practicable for the manufacturer to easily obtain, and yet at the same time to guard the question from the purchaser's standpoint so that the variation of the total delivery shall come close to the engineer's expectation. Cast-iron pipe can easily be kept within a variation of 4% in weight, and, on the total shipment, within a variation of 2% from the

standard. Where there is a factor of safety used of 500 to 600%, a difference in the variation of 1% or 2% in the weight of the individual pieces seems too small to lay stress on when granting it will aid the manufacturer in cost of production.

To come to those points which are most interesting to us, I may say that the practice in testing has gradually changed from tensile to transverse stress. Formerly we were required to furnish metal which would stand a tensile stress of 16 000, and in some cases 18 000 pounds per square inch. At present the usual requirement is that a 2" x 1" bar, 26 inches long, shall be capable of supporting a load of 1 800 to 2 000 pounds on supports 24 inches apart before breaking. It is also the custom to test finished pipes by internal hydraulic pressure. This test has not been carried to the point of destruction, but is limited generally to 300 pounds per square inch, and is really not a test of the normal strength of the casting, but simply intended to reveal latent defects that may not have been discovered in its prior examination. This hydraulic test is thus more a manufacturing than a scientific test. A good casting is thus very seldom broken, and then only on account of internal stresses.

It may be of interest to speak of the coating of cast-iron pipe, which was first done with what is known as the prescription of Dr. Angus Smith, of Glasgow. The lines laid down by Dr. Smith have been readjusted and entirely forgotten, although his name is still retained in specifications. The prescription called for a certain amount of chalk, and a certain amount of oil mixed with the tar, but engineers have now adopted a distillation of coal-tar, containing only sufficient oil to keep it elastic, which is applied at a temperature of 300°. When the Metropolitan Water Board of Massachusetts took up the subject of the supply of Boston and the surrounding cities, they made quite an elaborate examination as to how far the usual simple means of coating castings could be improved upon. Their conclusion was that the most economical and most effective way of coating cast-iron pipe is by the use of the standard distillation of coal-tar, as is practiced at present.

As to material, the iron we find most suitable for making pipes is an average quality. Of course we have to accept such

percentages of phosphorus and manganese as the mixture at each particular furnace dictates. We prefer to have the phosphorus over 0.5%, and do not object if it runs up to, or slightly exceeds, 1%. In fact, in all small castings it is quite essential to have the iron fluid, and to reduce the shrinkage so that the heads or sockets will not draw off of the top of the pipe. In the same way we find that .75 to 1.5% of manganese is desirable.

As to sulphur, we are the victims of circumstances; we would, of course, like to get rid of it altogether. Silicon should vary according to the weight of the pipe, from 1.25 % for heavy pipe to 2.25% for light pipe. There are, undoubtedly, a number of questions as to the value of pig iron for casting purposes, based upon other impurities and its mode of manufacture, which a Society like ours will gradually solve by a persistent comparison of analyses of the metal with the results obtained from its use. These, we must leave to the future.

# ON THE CONSTITUTION OF CAST IRON.

By Henry M. Howe.

Of the constitution of steel, and of the relation between its composition on one hand and its microscopic constitution and its physical properties on the other, we have already accumulated much knowledge. It appears to the writer that this knowledge offers a point of vantage from which to begin the study of the like relation between the composition and properties of cast iron. It is true that an important class of writers virtually insists that this knowledge must not thus be used, apparently holding that the very act of conferring a distinct name, cast iron, upon the iron-carbon compounds rich in carbon, has in some way operated to debar investigation along this line. But, as these writers have offered no reason in support of their injunction, save their own undoubted authority, and as the writer is no respecter of authorities, he proposes in the present paper to resume this banned mode of study.

Regarding cast iron as composed of two distinct parts, (1) its graphite, and (2) the remainder of the mass, which is metallic and may for brevity be called the matrix to distinguish it from the graphite, I advanced, in an earlier paper,* the principle that the strength of the whole mass ought to be (1) inversely as the quantity of graphite, which I regarded as necessarily a weakening and embrittling substance, and (2) dependent on the composition of the matrix, increasing or decreasing as this approached or receded in either direction from the composition of the strongest steel. While this composition, which should give the strongest matrix, should depend greatly on the quantity of the different elements such as silicon, phosphorus, manganese and sulphur, yet, were these constant, then the strongest matrix should be that with 1 or 1.20% of carbon, because that is the strongest steel, the strongest iron-carbon compound, as appears from many investigations.

In the present paper I propose to develop these ideas somewhat, and in particular to test them by presenting evidence as to the truth of two corollaries from them.

* "The Constitution of Cast Iron, with Remarks on Current Opinions Concerning It." *Trans. American Inst. Mining Engineers*, XXXI, p. 318, Feb., 1901

Graphiteless steel and graphiteless white cast iron seem to form a single continuous series, which we may call the pearlite series. That is, they seem to consist either (1) of pearlite alone, or (2) of pearlite plus ferrite, or (3) of pearlite plus cementite, according to whether they contain (1) 0.80% of carbon, or (2) less or (3) more than this quantity.

This has been shown to be true for our common carbon steels; and the present work assumes, for simplicity, that it is true for the whole range of graphiteless steels and graphiteless white cast iron. Naturally, this hypothesis should in time be tested.

Table 1 gives the microscopic constitution of this pearlite series on this hypothesis.

TABLE 1.

THEORETICAL CONSTITUTION OR MICROSTRUCTURAL COMPOSITION OF THE PEARLITE SERIES, SLOWLY COOLED STEEL AND WHITE CAST IRON.

| Name. | | Carbon. Per Cent. | Proximate Composition. Per Cent. | | | | |
|---|---|---|---|---|---|---|---|
| | | | Pearlite. | Excess* Ferrite. | Excess* Cementite. | Total Ferrite. | Total Cementite. |
| Steel. | Low Carbon. | 0 | 0 | 100 | 0 | 100 | 0 |
| | | 0.10 | 12 | 88 | 0 | 98.5 | 1.5 |
| | | 0.20 | 25 | 75 | 0 | 97.0 | 3.0 |
| | Medium Carbon. | 0.30 | 37 | 63 | 0 | 95.5 | 4.5 |
| | | 0.40 | 50 | 50 | 0 | 94.0 | 6.0 |
| | | 0.50 | 62 | 38 | 0 | 92.5 | 7.5 |
| | | 0.60 | 75 | 25 | 0 | 91.0 | 9.0 |
| | | 0.70 | 87 | 13 | 0 | 89.5 | 10.5 |
| | High Carbon. | 0.80 | 100 | 0 | 0 | 88.0 | 12.0 |
| | | 0.90 | 98 | 0 | 2 | 86.5 | 13.5 |
| | | 1.00 | 97 | 0 | 3 | 85.0 | 15.0 |
| | | 1.10 | 95 | 0 | 5 | 83.5 | 16.5 |
| | | 1.20 | 93 | 0 | 7 | 82.0 | 18.0 |
| | | 1.30 | 91 | 0 | 9 | 80.5 | 19.5 |
| | | 1.40 | 90 | 0 | 10 | 79.0 | 21.0 |
| | | 1.50 | 88 | 0 | 12 | 77.5 | 22.5 |
| | | 1.60 | 86 | 0 | 14 | 76.0 | 24.0 |
| | | 1.70 | 85 | 0 | 15 | 74.5 | 25.5 |
| | | 1.80 | 83 | 0 | 17 | 73.0 | 27.0 |
| | | 1.90 | 81 | 0 | 19 | 71.5 | 28.5 |

*"Excess Ferrite" means the ferrite in excess over the pearlite ratio, or in other words the free or structurally free ferrite which does not form part of the pearlite. "Total Ferrite" means the sum of this excess ferrite plus that contained in the pearlite. So, mutatis mutandis, with "Excess Cementite" and "Total Cementite."

| Name. | Carbon. Per Cent. | Proximate Composition. Per Cent. | | | | |
|---|---|---|---|---|---|---|
| | | Pearlite. | Excess* Ferrite. | Excess* Cementite. | Total Ferrite. | Total Cementite. |
| White Cast Iron. | 2.00 | 80 | 0 | 20 | 70.0 | 30.0 |
| | 2.10 | 78 | 0 | 22 | 68.5 | 31.5 |
| | 2.20 | 76 | 0 | 24 | 67.0 | 33.0 |
| | 2.30 | 74 | 0 | 26 | 65.5 | 34.5 |
| | 2.40 | 73 | 0 | 27 | 64.0 | 36.0 |
| | 2.50 | 71 | 0 | 29 | 62.5 | 37.5 |
| | 2.75 | 66.75 | 0 | 33.25 | 58.75 | 41.25 |
| | 3.00 | 62.5 | 0 | 37.5 | 55.0 | 45.0 |
| | 3.25 | 58.25 | 0 | 41.75 | 51.25 | 48.75 |
| | 3.50 | 54.0 | 0 | 46.0 | 47.5 | 52.5 |
| | 3.75 | 49.75 | 0 | 50.25 | 43.75 | 56.25 |
| | 4.00 | 45.5 | 0 | 54.5 | 36.25 | 63.75 |
| | 4.50 | 37.0 | 0 | 63.0 | 32.5 | 67.5 |

Fig. 1 sketches the physical properties of this series in a general way. The lines here given must be taken simply as indicating roughly the normal properties of the materials. It should of course be applied with great caution. For instance, we know that the strength of steel can be more than doubled by varying the rate at which it is cooled. Other causes also change its strength greatly.

Gray cast iron, together with those high-carbon steels which contain graphite (though this is usually in very small quantity), form a second series, which we may call the graphito-pearlite series. That is to say, they consist essentially of (1) their matrix, which is some one member of the pearlite series, plus (2) their graphite.

By their "Matrix" I mean the whole of their ferrite and cementite, *i. e.*, their metallic part. This forms a sort of metallic matrix, in which is imbedded the wholly non-metallic graphite practically as a foreign body. This matrix, in that it consists, according to our hypothesis, of ferrite and cementite, is identical in composition and constitution with some one member of the pearlite series, because every member of that series is thus composed of ferrite and cementite. Thus these graphitiferous steels and cast irons, as has just been said, consist, according to our hypothesis of (1) a metallic matrix of ferrite and cementite, identical in each case with some one member of the pearlite series, and (2) their graphite.

*Constitution and Properties of Mottled and Gray Cast Iron, the Graphito-Pearlite Series.*—In the paper already referred to I made a first attempt to reconcile the constitution and properties of the pearlite series. That task was a relatively simple one; the parallel task in our present case of the graphito-pearlite series is more difficult, perhaps not so much because we as yet only conjecture the constitution of this series and because our knowledge of its properties is extremely fragmentary, as because of the greater complexity of its constitution. Table 1 shows that, as regards the relation between composition and constitution, we may for our present purpose assume that the pearlite series has but a single variable, the carbon-content; it is, as it were, a

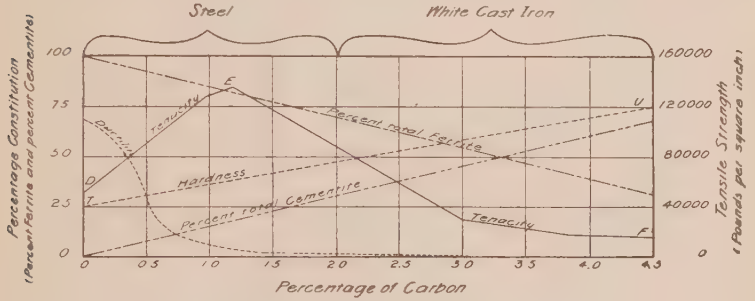



FIG. 1. Physical Properties and Assumed Microscopic Constitution of the Pearlite Series. Graphiteless Steel Slowly Cooled, and White Cast Iron.

NOTE. By "Total Ferrite" is meant both that which forms part of the pearlite and that which is in excess of the pearlite, taken jointly. So with the "Total Cementite."

single line, each percentage of carbon corresponding to a fixed and definite percentage of ferrite and of cementite respectively. The graphito-pearlite series on the other hand, has two variables, the carbon-content, and the distribution of that carbon between the conditions of free graphite and of combination as cementite.* The series thus is not a single line but a field, since for each percentage of total carbon the distribution between the graphitic and cementite states varies widely; and with each of these

*In the present paper I discuss the constitution and properties of only slowly-cooled material supposed to consist of pearlite and ferrite or cementite, together with graphite. The carbon present, then, will be in every case either in the condition of graphite or of combined, i. e., cementite carbon. "Combined carbon," then, in this article refers to cementite carbon, which for uniformity I have spoken of throughout as combined carbon.

variables, total-carbon-content and carbon-distribution, the constitution varies.

Let a single instance of each of these variations serve as an example, adopting our present hypothesis:

(1) *Total Carbon Constant, Graphite Varies.*—A cast iron with 4% of carbon may have all, or none, or any intermediate part of that carbon in the form of graphite. If the whole 4% is graphite, then the cast iron as a whole is a conglomerate of (1) that 4% of graphite plus (2) a metallic matrix forming 96% of the whole mass, and itself consisting of pure ferrite. If, on the other hand, only 1% of that carbon is graphite and the remaining 3.00% is combined as cementite, then the cast iron as a whole is a conglomerate of (1) 1% of graphite, and (2) a metallic matrix forming 99% of the whole mass, and itself containing $(4-1) \times 100 \div 99 = 3.03\%$ of carbon, and therefore by Table 1 consisting of 54.55% of ferrite and 45.45% of cementite, or say 55 and 45%. To recapitulate this:

TABLE 2.

| Name of Cast Iron | Per Cent of Carbon. | Constitution. | | | |
|---|---|---|---|---|---|
| | | Graphite. Per Cent. | Metallic Matrix. Per Cent. | Constitution of Matrix. | |
| | | | | Ferrite. Per Cent. | Cementite. Per Cent. |
| Very open gray ..... | 4 | 4 | 96 | 100 | 0 |
| Mottled ..... | 4 | 1 | 99 | 55 | 45 |

(2) *Graphite Constant, Total Carbon Varies.*—Let us next assume that there is 1% of graphite, and that the total carbon varies from 3 to 4%. With 3% of carbon, the constitution is (1) free graphite 1%, (2) metallic matrix 99%, itself containing $(3-1) \times 100 \div 99 = 2.02\%$ of carbon, and therefore, by Table 1 consisting of ferrite 69.7%, cementite 30.3%, or say 70 and 30%.

With 4% of carbon, on the other hand, the constitution is (1) as before 1% of free graphite, and (2) as before 99% of metallic matrix, consisting of 55% of ferrite and 45% of cementite. To recapitulate this:

TABLE 3.

| Name of Cast Iron. | Per Cent of Carbon. | Constitution. | | | |
|---|---|---|---|---|---|
| | | Graphite. Per Cent. | Metallic Matrix. Per Cent. | Constitution of Matrix. | |
| | | | | Ferrite. Per Cent. | Cementite. Per Cent. |
| Mottled | 3 | 1 | 99 | 70 | 30 |
| Mottled | 4 | 1 | 99 | 55 | 45 |

*Correspondence Between the Constitution and Properties of the Series.*—Rather than seek to cover this whole area of the graphito-pearlite series, let us consider a single line in that area; for the principles which apply to that line should apply to the area as a whole. Let us select for our line that of total carbon 4%, and graphite varying from 0 to 4%. This is perhaps the most useful line for our study, because it passes about through the average of our commercial pig irons, a large proportion of which contain not far from 4% of carbon, *i. e.*, are approximately saturated with carbon in the blast-furnace. These irons, or the most important of them, then, differ relatively little in their total carbon-content, but they differ very greatly in the distribution of that carbon between the conditions of graphite and of combined carbon.

Let us see how far the properties of such a series of cast irons, so far as we know them, agree with our hypothesis, that each member of this series is essentially a conglomerate of (1) the corresponding member of the pearlite series as a metallic matrix, and (2) graphite.

To this end let us try to forecast the properties of this series of cast irons all containing 4% of total carbon, but with the distribution of that carbon between the state of graphite and that of cementite varying. All the carbon that is not in the state of graphite is assumed to be in the state of combined carbon in the cementite of the matrix; which assumption fixes the constitution and composition of the matrix for every assumed percentage of graphite.

In order to arrive at this forecast, we in effect ask, in what

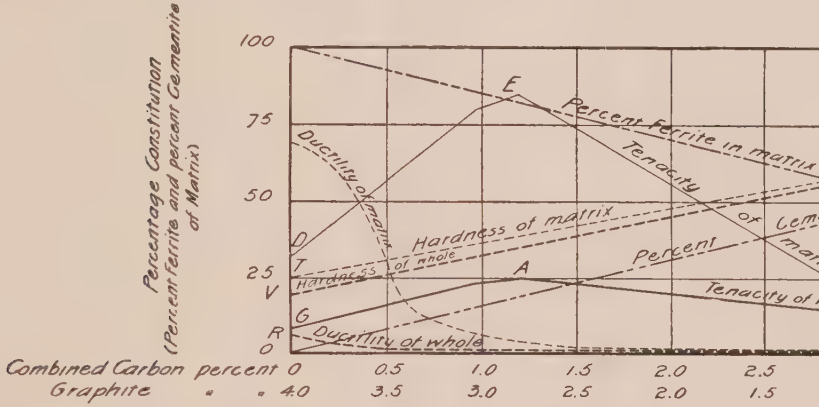
direction and how much should the properties of the cast iron as a whole differ from those of its matrix? And we answer this on the assumption that the difference between the properties of the cast iron and those of its matrix is solely that due to the presence of a mechanical admixture of graphite in that matrix.

We next ask, as a means of verifying our hypothesis, how far does this forecast agree with our knowledge of the properties of such cast irons?

Fig. 2 may facilitate such a forecast. First the lines which in Fig. 1 show the physical properties of the pearlite series and those showing the proportion of ferrite and of cementite in that series are here reproduced, with the same lettering as in Fig. 1. These lines are supposed to show the constitution and properties of the matrix, for varying composition, *i. e.*, for varying carbon-content, of that matrix on the assumption that the properties of the matrix itself, its tensile strength, ductility and hardness, are the same as those of a graphiteless steel or a graphiteless white cast iron which has the same carbon-content and hence the same microscopic constitution as the matrix. For instance, the mottled cast iron of the second line of Table 3 is represented by ordinate W of Fig. 2. Table 3 shows us that the constitution of its matrix is ferrite 55%, cementite 45%. Turning now to Table 1 we find that this is the constitution of white cast iron containing 3 per cent of total carbon, all of which is combined as cementite. Accordingly, the lines in Fig. 2 which give the constitution of this matrix ("Per cent Ferrite in matrix" and "Per cent Cementite in matrix") cut this ordinate W at 55% and 45% respectively. Next, Fig. 2 assumes that the properties of this matrix itself, as distinguished from the cast iron which contains it are the same as the properties of such a white cast iron with 55% ferrite and 45% cementite; hence the line "Tenacity of matrix" cuts ordinate W at the same height as that at which it cuts the 3% carbon ordinate in Fig. 1; and so with the lines "Hardness of matrix" and "Ductility of matrix."

These three lines, then, give us the tenacity, ductility and hardness, on the present hypothesis, of the matrix of each cast iron of this series shown in Fig 2, as a basis for forecasting the properties of the cast irons themselves regarded as a conglomerate of (1) their matrix and (2) graphite; the properties of this

| Name of matrix | Low carbon steel | Medium carbon steel | High carbon steel | Ho… Wh… |
|---|---|---|---|---|
| Name of the cast iron *i. e.*, of the whole | Very open gray or very graphitic cast iron | | Close gray cast iron | Mott… cas… iro… |




| | | | | | |
|---|---|---|---|---|---|
| Strength | | | Weak | Strongest | |
| Ductility | | | Least brittle | | |
| Hardness | | | Softest | Soft | Harder |
| Because of its | strength and ductility | the iron is suited to uses in which it will have to undergo or resist | shock | moderate shock the iron is suited to most engineering purposes | |
| | hardness | | much machining in preparation, but little abrasion in use | moderate machining in preparation, not excessive abrasion in use | |

FIG. 2. Physical Properties and Assumed Microscopic Constitution of Cast Iron containing 4 p…
Affected by the Distribution of that Carbon between the Combined and Graphitic S…

conglomerate, *i. e.*, of the several cast irons each taken as a single conglomerate whole, are represented by the lines "Tenacity of whole," "Ductility of whole" and "Hardness of whole." The forecast which will here be made will be a first attempt to determine the position of these three lines, as inferred from our hypothesis.

Above the diagram is a line showing the trade name or grade of the cast iron as a whole, and above it a second line showing the nature of the matrix by what we may call its trade name. In other words, this line gives the trade names of the different classes of steel or cast iron to which the matrix corresponds in constitution, in different regions of this series. For instance, the words "Medium carbon steel" in this line mean that the matrix of the cast irons beneath these words has the same ultimate chemical composition and therefore, on our hypothesis, the same microscopic constitution, *i. e.*, the same percentage of ferrite and cementite, and also the same physical properties, which medium carbon steel has.

Below the diagram are given the observed properties of the different kinds of cast iron here represented, and the kinds of uses to which they are fitted.

## ATTEMPT AT A FORECAST.

Let us first ask "How should the interspersing of flakes of graphite affect the properties of the several members of the pearlite series?" Or, in brief, "Should the tenacity, hardness and ductility lines of the graphito-pearlite series lie above or below those of the pearlite series, and how far above or below?"

At the right end of the diagram, where graphite is zero, each line for the properties of the cast iron as a whole should touch the corresponding line of the matrix; *i. e.*, the properties of the matrix and of the whole are identical because the matrix is the whole. This fixes the right-hand end of each line.

Let us consider the position of the remainder of the line for tensile strength, and then for ductility and hardness.

*Tensile Strength.*—As graphite is itself a very weak substance, interspersing flakes of it through our metallic matrix should be qualitatively like setting weak links in a strong chain; or, since the flakes of graphite are not continuous, like here and

there notching or sawing part way through certain links of a chain. It needs no persuasion to show that these continuity-destroying flakes should weaken the mass, and that the weakening effect should increase with their quantity. We admit then that the tenacity line for the cast iron must throughout lie beneath that of the matrix.

How far below D will its left-hand end lie? From our general knowledge of the facts we may put the strength of this cast iron, *i. e.*, the position of G, at 12 000 pounds per square inch, as the tensile strength of a very weak cast iron. At first we should suppose that as, on passing to the right, the proportion of graphite decreases continuously and regularly, so should its influence, and so in fine should the gap between the tenacity line for the cast iron and that for the matrix decrease regularly; that is to say, the distance between the two lines should be a regularly decreasing percentage of the ordinate of line DEF: in short, we should at first expect a *regularly decreasing-per-centage gap*. But this assumption would be too far wrong, as appears clearly even on the superficial examination here possible. Let us look a little into this question.

The weakening effect of graphite is due primarily to its breaking up the continuity of the metallic matrix. Assuming first, for the moment, that the influence of the graphite is proportional to the area of the flakes of graphite, and that the area is proportional to the percentage of graphite, and assuming as we have, that the left-hand cast iron of our series with 4.00% of graphite and no combined carbon, has a tensile strength of 12 000 pounds per square inch against 50 000 pounds in case of pure ferrite, then in this case the weakening effect of our 4% of graphite is 50 000 — 12 000 = 38 000 pounds per square inch or 76%. Supposing the influence of graphite to remain proportional to this, what should the strength of the cast iron with matrix of composition E be? The percentage of graphite in this iron is 2.80. The weakening effect of this quantity of graphite should be $76 \times 2.80 \div 4.00 = 53\%$. The tensile strength of the matrix itself should, from Fig. 2, be 135 000 pounds per square inch. As the tensile strength of the cast iron as a whole is to be 53% below that of the matrix, it should be $135\,000\ (100 - 53) \div 100 = 63\,500$.

This, then, is the first result of our attempt to test our hypothesis. Is this result sufficiently near the truth to support the hypothesis? Manifestly not. From all our knowledge of the subject, it is highly improbable that cast iron of this composition, combined carbon 1.20%, graphite 2.80%, has so great tensile strength.

In view of this, we next consider whether in making our calculation, we have not omitted some important consideration, which should affect our result, and bring it closer to the truth. Many such considerations may exist; three certainly suggest themselves. Of these the first two have to do with the rate at which the influence of the graphite increases, while the third has to do with the variations in the effect of the graphite due to its changing composition and properties of the matrix itself. We will now take these up consecutively.

*The Influence of the Graphite Increases Less Than Proportionally to its Percentage.*—For simplicity I have thus far assumed that the influence of the graphite on tensile strength ought to be proportional to the percentage of that substance. This might be true in case the dimensions of the individual flakes of graphite remained constant, so that a cast iron with 4% of graphite differed from one containing 2% simply in having twice as many flakes, but of the same area and thickness. If, however, the increase in the quantity of graphite was accompanied by a change in the size or shape of the individual flakes, then, while the influence of the graphite should still increase as the quantity of graphite increased, so that the influence of the graphite should be as some power of its percentage, yet this power should not be unity.

Two considerations go to show that this power should be less than unity, or in other words, that the first small quantity of graphite ought to have a more powerful effect, per cent for per cent, than later additions. These considerations are as follows:

If we were to assume that, as the percentage of graphite increased the number of flakes of graphite remained constant, and that each flake or sheet grew simultaneously both in thickness and in width, so as to preserve the initial relation between its thickness and its area; then the area of the flakes should

increase as the two-thirds power of the percentage of graphite. Now it might be reasonably held that the weakening effect of these flakes of graphite should be more nearly proportional to the area of the several flakes, than to their volumes; and hence that, on this assumption, the influence of the graphite should increase more nearly as the two-thirds power of the percentage of graphite than directly as that percentage.

As it is probable that, as the graphite increases, there is an increase both in the number of flakes and in the area of certain of them, this consideration points to a power between two-thirds and unity.

The second of our three considerations is that, once our matrix has been weakened by the notching effect of the presence of a certain given quantity of graphite, a further increase in this notching through an increase in the percentage of graphite should not have a proportionally great effect. This may be made clearer by considering the effect of a like increase of notching on the strength of a strip of cloth under tensile stress.

Thus, if we were to cut above the notch A in Fig. 3 a second notch just like it, this second notch should not materially add to the weakening effect caused by the first. Nor, if a second notch should be cut at the opposite edge of the cloth, on the same horizontal line as A, is it clear that this second notch would add greatly to the effect of the first. It would be like filing on one side of a given link in a chain, a notch exactly like that one already filed on the other side of that link. The second notch would certainly weaken the chain somewhat, but it would not double the effect of the first notch. Here, then, is a second reason for holding that the weakening influence of the notching caused by the presence of flakes of graphite should increase according to some power less than unity, of the percentage of graphite itself.

As the immediately preceding consideration pointed to a power between two-thirds and unity, and as this last consideration makes for a further lowering of the power, let us adopt temporarily two-thirds as this power, and again calculate on this new assumption the tensile strength of cast iron with matrix E. Doing this we find that the presence of 2.80%

graphite lowers the tensile strength by 60%, which gives a calculated tensile strength of 54 000 pounds per square inch.*

But this is still too high; it is not probable that cast iron of this composition, combined carbon 1.20%, graphite 2.80%, has such great strength. So we turn to our next consideration, based on the changing constitution and properties of the matrix.

*Influence of the Properties of the Matrix on the Weakening Effect of the Graphite.*—The third consideration to which attention is asked is that the weakening effect of the graphite should increase with the carbon-content of the matrix, which the graphite weakens through breaking up its continuity.

This breaking up of continuity is in effect like nicking or notching our matrix. But all our experience shows us that the effect of notching varies greatly with the nature of the material notched. In two like rods, one of oak and the other of glass, file a like sharp notch. The strength of the oaken rod is hardly affected, while that of the glass rod is lessened out of all proportion to the quantity of material removed by filing, so that the rod is easily snapped by the fingers. The effect is like that of notching one edge of a strong roll of cloth (Fig. 3). Suppose that such a piece of cloth is firmly gripped at its two ends by the frames D and E, and that these frames are pulled away from each other; the cloth comes under tensile stress, which we may assume is evenly distributed, and represented by the arrows. Because of the notching, the material at A, which, but for the notching would have to resist only stress dd, now has in addition to resist a large part of stresses aa, bb and cc. It yields under the concentration of stress, as then does its next successor when in turn exposed to the stress, and so the piece breaks down in detail. But the pliant oak or lead or copper is affected rela-

* The details of the calculation are as follows: We have already calculated that the weakening effect of 4% of graphite was 76%, *i. e.*, that it brought the tensile strength of the cast iron as a whole 76% below that of its matrix. Following the assumption that the weakening effect of the graphite is as the two-thirds power of the percentage of graphite, the weakening effect of the 2.80% of graphite corresponding to point E should be found by the proportion:

$$\overline{4}^{\frac{2}{3}} : \overline{2.80}^{\frac{2}{3}} = 76 : x = 60\%.$$

The tensile strength of this cast iron with 2.80% of graphite and 1.20% of combined carbon should be 60% less than that of its matrix; this we have already seen has a tensile strength of 135 000 pounds per square inch; so that the tensile strength of this cast iron should be $135\,000 \times 40 \div 100 = 54\,000$ pounds per square inch.

tively slightly by such notching, and so is the pliant ferrite, as we know it in the low-carbon steels and wrought irons.

Let us apply this to the pearlite series. The 1.20% carbon steel represented by point E in Fig. 2 is a relatively rigid, unyielding material, with but little ductility; the pure ferrite represented by D is extremely ductile. The former is nearer to glass, the latter to oak or copper in its properties. Hence it is to be expected that a given degree of notching should remove a far greater fraction of the strength of the former than of the latter. Hence it would not be at all surprising were we to find that the strength of the cast iron corresponding to E instead of being 100 — 60 = 40% of E, or 54 000 pounds, should actually have only say 30% of E, or say 135 000 x 0.30 = 40 - 500 pounds per square inch. Following this reasoning, the tensile strength line GAF has been drawn. It is clearly conjectural; yet it appears to fit fairly well both the facts, so far as observed, and our hypothesis.

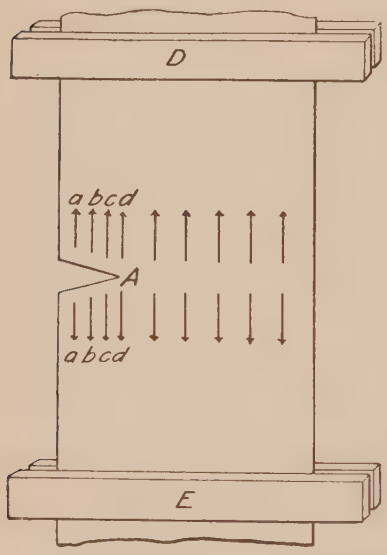


FIG. 3. Effect of Notching on Tensile Strength.

*Tensile Strength of Malleable Castings.*—The high tensile strength of malleable cast iron castings agrees easily with our hypothesis, and with the view that the size of the particles of graphite as well as the quantity of graphite should influence the tensile strength greatly.

Indeed, the fact that these malleable castings have three features, each of which, according to our hypothesis, ought to lead to high tensile strength, goes to make the great strength of these castings a support to this hypothesis. In other words, the fact that these castings, which according to our hypothesis ought to be strong, actually are very strong, in itself supports the hypothesis.

The three features of the constitution of malleable cast iron to which I refer are the following:

(1.) Its total carbon is in general much less than that of common gray cast iron.

(2.) In many of the reported analyses the combined carbon is not far from 1.20%.

(3.) Its flakes of graphite are extremely fine, whereas those of gray cast iron are very much coarser.

Now, reasons (1) and (2) are not in themselves sufficient to account readily for the high tensile strength of these castings. Hence we refer part of this high strength to the third reason, the fineness of the graphite itself.

*Ductility.*— (1) Qualitatively. But little persuasion is needed to convince one that interspersing flakes of a non-extensible, *i. e.*, non-ductile (though indeed flexible), substance like graphite through the metallic matrix should lessen its ductility. Ductility implies flow and rearrangement of the particles of the metal, which, as the mass is drawn out, should perform the "ladies' chain" figure of the quadrille, a given particle relinquishing its grasp on some of its initial neighbors and instead attaching itself to new intimates. The least consideration of what takes place during elongation beyond the elastic limit gives us this idea; and the beautiful experiments of Ewing and Rosenhain show that this conception is essentially true. Conceive a ductile metallic ball one inch in diameter gradually drawn out into a thin wire one-hundredth of an inch in diameter and 555 feet long. When we begin to elongate the sphere and before the elastic limit is reached, each small particle (or let us say each molecule, whichever conception is the easier) may be supposed to elongate proportionally, like the particles of a rubber ball, each molecule retaining its initial contact with all of its neighbors. Hence the return to the initial shape when the deforming stress is released. But when the elastic limit is passed we are forced to suppose that some at least of the molecules part company with some of those with which they were initially in contact, and attach themselves to new ones. Thus our sphere had a surface of 3.14 square inches; our wire has a surface of 209 square inches, or 66 times as much. Now, were our new and great surface composed exclusively of the initial

surface molecules, each so distorted as to offer 66 times as much surface as initially, and were this same distortion to extend through the mass, then on releasing the stress these molecules should all resume their initial dimensions, and so should the mass as a whole return to its initial spherical shape; in short, the elastic limit should not have been passed. The very fact that it has been passed indicates that the individual molecules have undergone something more than the deformation of those of a rubber ball; and this "something more" is, I take it, a regrouping, a ladies' chain, a divorce and remarriage.

To bring about this supposed 66-fold increase of the surface, many molecules which were initially submerged below the surface must have emerged to the surface. Conceiving for the moment that each molecule is a perfect cube, and that all our cubes are laid square, it follows that every submerged molecule has contact with a neighbor on six of its faces, while every emerged one is in contact with only five of its neighbors: in other words, that that one of its six faces which forms part of the surface of the sphere is without such contact. And no matter what shape we assign to the molecules, a like conception forms in our minds. Now the 66-fold increase in the area of surface, *i. e.*, in the number of surface molecules, implies that each of an enormous number of molecules which were initially submerged and in contact with six neighbors, in emerging and reaching the present state of contact with only five neighbors, must at the very least have parted company with one of its initial neighbors; and we cannot resist the inference that this parting has been simply one of many steps, of many linkings and unlinkings, probably thousands of them, during the drawing out from the initial state of a sphere into the final state of a wire.

Our conception that, while distortion within the elastic limit does not imply regrouping of the particles, elongation beyond that limit does, is strengthened by considering the distortion of cotton. The loose wool of cotton* may be pulled by the hand into every kind of shape, and it is practically wholly

* By the "wool of cotton," often called "cotton wool" I refer to the loose cotton before spinning, as distinguished from cotton thread and cotton cloth. It is similar to "cotton batting" and is familiar to all as the soft fluffy, pink, woolly substance which jewelers put loosely into small boxes to hold small pieces of jewelry

inelastic; it does not return to its initial shape. This is because its fibers are so loosely interhooked that they readily give up their old loves for new. As ductility is one manifestation of the power of being elongated beyond the elastic limit, a lump of such cotton wool may be regarded as extremely ductile in the sense that, while wholly inelastic, it can be distorted or drawn out in any direction and to a very great degree, and that when thus drawn out it retains its new shape instead of returning elastically to its original shape. Now, we may refer this quasi-ductility to the facility with which the unclosed hooks of the loose cotton release one another and immediately hook into new ones. But let us spin that cotton into thread and thereby lock hook into hook so firmly that this divorce and remarriage is no longer possible, and ductility is nearly destroyed. The thread can undergo very little distortion beyond the elastic limit; and this we may say is because remarriage is impossible, and this in turn because the interlocking is so firm that if divorce occurs it destroys the hooks themselves, without which remarriage cannot occur. This may aid our conception that ductility, *i. e.*, the power of being elongated past the elastic limit, of taking great permanent set, implies power of the particles to divorce and remarry.

Now this remarrying power must of course be interfered with by the presence of any foreign body like graphite. Any given molecule can during distortion readily part from its initial neighbors; but how can it embrace a new one from which it is separated by a layer of graphite, even if that be ever so thin?

How then is slag-bearing wrought iron ductile? It is ductile along the grain, because under distortion parallel with themselves the rods of slag do not interfere with remarriage. But it is brittle across the grain because they do interfere with transverse remarriage. This case of wrought iron, then, rather supports than opposes this idea of the necessarily embrittling effect of graphite.

Admitting, then, that graphite should lessen ductility, and provisionally and for the sake of simplicity that this lessening should be proportional to the percentage of graphite (it is of course also dependent upon its distribution), the ductility line for the cast iron should throughout lie beneath that of the matrix, with regular percentage decrease of the gap from left to right.

(2) Quantitatively. Unfortunately there is little published data as to the ductility of cast iron. To fix our ideas let us assume that the most ductile of our present series, *i. e.*, the left-hand end of the diagram, has an elongation of 2.5%, as against 40% for pure ferrite: then plotting on the regularly-decreasing-gap plan, we have the ductility curve RS for the series of cast irons given in Fig. 2.

The assumption of 2.5% as the elongation of the left-hand member of our series, the cast iron with 4% graphite and combined carbon zero, is simply the best guess which I can make from a general knowledge of the facts.

*Hardness.* Certainly the presence of graphite should make the cast iron as a whole softer than its matrix, so that the hardness line for the cast iron should throughout lie beneath the hardness line TU of the matrix. In default of actual data we may provisionally fix the left-hand end of the line by assuming the hardness of ferrite as 4.5 of Mohr's scale, and that of graphite as 1.5, and further assuming that the hardness of our left-hand cast iron will be proportional to its percentage of ferrite and graphite, or $(96 \times 4.5 + 4 \times 1.5) \div 100 = 4.38$. This gives us point V; then plotting the remainder of the line on the regularly decreasing-gap plan as a first extremely rough approximation, we get the hardness curve for the series of cast irons, VU, plotted in Fig. 2. That is to say, at the right-hand end of the diagram where graphite is zero, the hardness of the cast iron as a whole should equal the hardness U of the matrix simply because the cast iron consists solely of that matrix: hence the hardness of this right-hand cast iron should be U.

Next, if we assume that the softening influence of the graphite is directly proportional to the percentage of graphite, then the gap between the lines TU the assumed hardness of the matrix, and VU the hardness of the cast iron as a whole which we are seeking, should decrease regularly as we pass from left to right. That is to say, the gap between the two lines should be a regularly decreasing percentage of the ordinate of TU. Hence, since TU has been assumed to be straight, VU also should be straight.

*How do these Inferred Lines Agree with the Facts?*—The lines for the tensile strength, ductility and hardness of cast iron as a whole which we have now drawn in Fig. 2 are those which are

indicated by our hypothesis, the hypothesis that the properties of cast iron are due in very important part to its consisting of a mechanical mixture or conglomerate of (1) graphite and (2) a matrix of ferrite and cementite, which matrix is itself in each case some one member of the pearlite series. Rather, these lines are a first approximation to those which our hypothesis calls for; a first approximation only, because we have had to make many assumptions on scanty foundation of evidence, and in making some of these assumptions we have for simplicity ignored important considerations. In view of this, we cannot expect a close agreement between our lines and the facts; all that our hypothesis demands at present is that there shall be no glaring disagreement between these lines and our observed facts.

To bring together the existing data so as to test these lines, these inferences from our hypothesis, would in itself be an extremely serious task; and even if all available data were assembled, they would probably be found extremely defective, so that further great labor would be needed to supply the defects.

But we may here in a general way ask how these lines agree with our general knowledge of the properties of cast iron as influenced by its percentage of combined and of graphitic carbon.

This question each reader must answer for himself. To the writer this agreement seems as good as is to be expected in view of the conditions. It is not to be pretended that there is such an agreement as in and by itself constitutes a firm or even an important support for the hypothesis; all that is claimed is that there is no marked disagreement between these lines and our general knowledge, no disagreement too great to be referred reasonably to the other important variables, such as the influence of phosphorus, sulphur, silicon, etc., the rate of cooling, and the considerations which in this paper have for simplicity been purposely ignored.

A crude but easy test of our hypothesis is to compare the lines in the diagram with the tabular wording arranged above and below the diagram itself, showing the "Name of matrix," "Name of the cast iron," "Strength," "Ductility," "Hardness," etc., and indicating the prominent uses of the various classes of

cast iron. It seems to me that the grading, properties, and uses here summarized are (1) wholly in harmony with the teaching of our diagram and hence with our hypothesis, and (2) a fair summarized statement of our knowledge of the facts. In short, it seems to me that, judged by this comparison, there is a fair agreement between our hypothesis and the facts.

*Probable Composition of the Strongest Cast Iron.*—Following out our hypothesis that cast irons are essentially a conglomerate of graphite with a metallic matrix, let us next ask what composition we should expect to give the greatest tensile strength.

In considering Fig. 2, we supposed that, in a series of cast irons all containing 4% of total carbon but varying in the distribution of that carbon, the strongest ought, on our hypothesis, to contain 1.20% of combined carbon. Like reasoning applied to any other series of given total carbon, leads to a like inference that the maximum tensile strength for that series should be reached with 1.20% of combined carbon. Let two examples illustrate this:

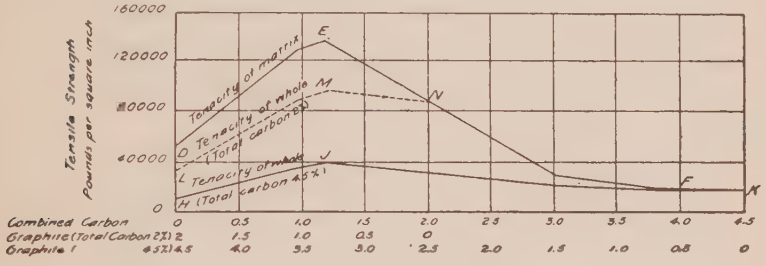
Let us try to draw in Fig. 4 a line to represent the tensile strength to be expected on this hypothesis in a series of cast irons all containing 4.50% of total carbon, but with the distribution of that carbon varying from graphite 0 combined carbon 4.50 to graphite 4.50 and combined carbon 0.

With graphite 0 and combined carbon 4.50%, the tensile strength curve touches that of the pearlite series at K by the same reasoning which caused that of our 4.00% carbon series in Fig. 2 to cut it at F. With graphite 4.50% and combined carbon 0, the matrix of our cast iron as before should be pure ferrite, through which should be distributed the flakes representing this 4.50% of graphite. Because this quantity of graphite is larger than that corresponding to point G in Fig. 2, the tensile strength of this present cast iron should be below G; and for like reason the whole curve should be below GAF of Fig. 2, and yet it should be of the family of GAF, reaching its maximum point at 1.20% of carbon. Without quantitative attempt we may roughly sketch the expected tensile strength curve as HJFK in Fig. 4.

In like manner we may draw in this same Fig. 4 the tensile strength curve of a series containing 2.00% of total carbon.

This curve should be somewhat like LMN in Fig. 4. For with graphite 0, combined carbon 2%, the line should cut DEF at N: the left-hand end should be above G, because only 2% of graphite is present instead of the 4% which G represents; let us set it as roughly midway between D and G, at L. Then, adopting the general family shape of GAF and HJK, the tensile strength curve for the 2.00% total-carbon series should be LMN, as here drawn.

And so no matter what the total carbon, the tenacity should fall in a curve of this family; and, moreover, the less the total carbon the higher should the tenacity line as a whole lie. From this we may draw the following corollaries:




Percentage Composition.

FIG. 4. Expected Influence of Quantity and Distribution of Carbon on the Tensile Strength of Cast Iron containing (HJFK) 4.50 per cent. of Carbon, and of Cast Iron (LMN) containing 2 per cent. of Carbon.

Corollary 1. For given total carbon the strongest cast iron should be that with from 1 to 1.20% of combined or cementite carbon, and the tensile strength should decrease as the combined carbon departs in either direction from this percentage.

Corollary 2. For given combined carbon the strongest cast iron should be that with the least graphite.

Corollary 3. The strongest cast iron of all should be that with 1 to 1.20% of combined carbon and 0.80% of graphite, if we arbitrarily draw the line between steel and cast iron at 2.00% of total carbon. For on one hand the matrix of such a cast iron would have the composition of greatest strength, and on the other hand, the quantity of graphite by which this matrix is weakened is the smallest which any cast iron with such a matrix can have and yet be a true cast iron, if we follow the arbitrary

classification that any pure iron-carbon compound is cast iron if it contains over 2% of total carbon. I purposely say "pure iron-carbon compound," because the boundary between steel and cast iron may be shifted greatly by the presence of elements other than carbon.

Indeed, it is perhaps hardly necessary to go through the sort of crude demonstration here given. The result should follow directly from the hypothesis. The strongest cast iron should be that with the strongest matrix, which we hold corresponds to about 1.20% of carbon, and with the smallest quantity of the weakening graphite, which by difference is 0.80%. That is to say, we have assumed that nothing can properly be called cast iron which contains less than 2% of total carbon; this we have taken as a matter of definition. Hence the cast iron which, along with the matrix of maximum strength, 1.20% combined carbon, shall have the minimum quantity of the weakening substance graphite, is that with $2 - 1.20 = 0.80\%$ of graphite.

*Test of Corollary* 1.—Of course the tensile strength should be affected by other variables than that here considered, such as the rate of cooling, the size of the particles of graphite, the influence of other elements such as phosphorus and silicon. Hence it is not to be expected that any of these corollaries should be complied with very accurately. But, to get some sort of first crude test, I have plotted in Figs. 5 to 13 all the data which I could find bearing upon corollary 1. For this purpose I have taken all the cases which I could find in which the tensile strength of cast iron, and also its percentage of combined and of graphitic carbon, were given.

From among these many had to be rejected for the reason that there was strong doubt whether the composition given really corresponded to the tensile strength. For instance, in many cases the composition of a pig iron, or unremelted cast iron as it came from the blast-furnace, was given, and along with it the tensile strength of a test specimen made by remelting that pig iron. But this remelting would probably change the composition greatly; so that the tensile strength given would represent a composition not only very far different from that given, but indeterminate. All such data had to be rejected.

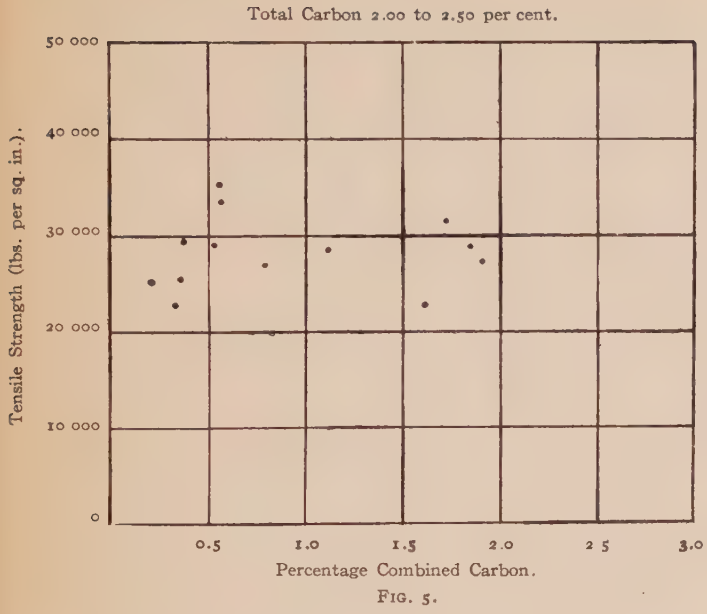



FIG. 5.

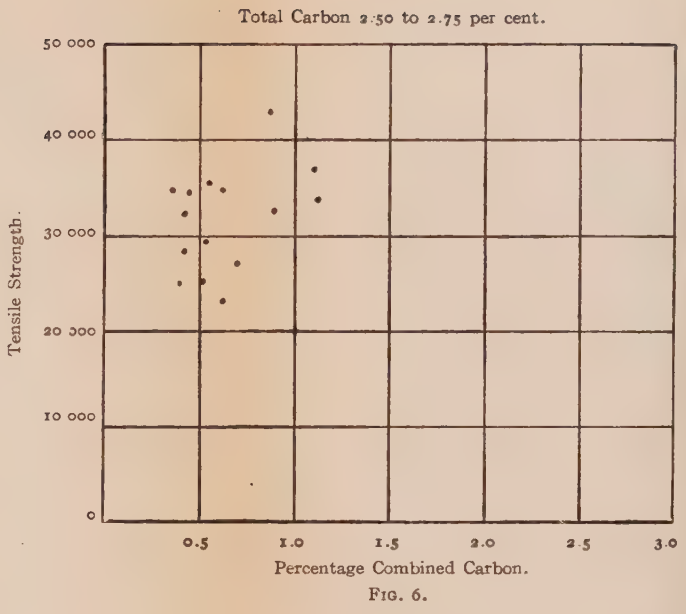



FIG. 6.

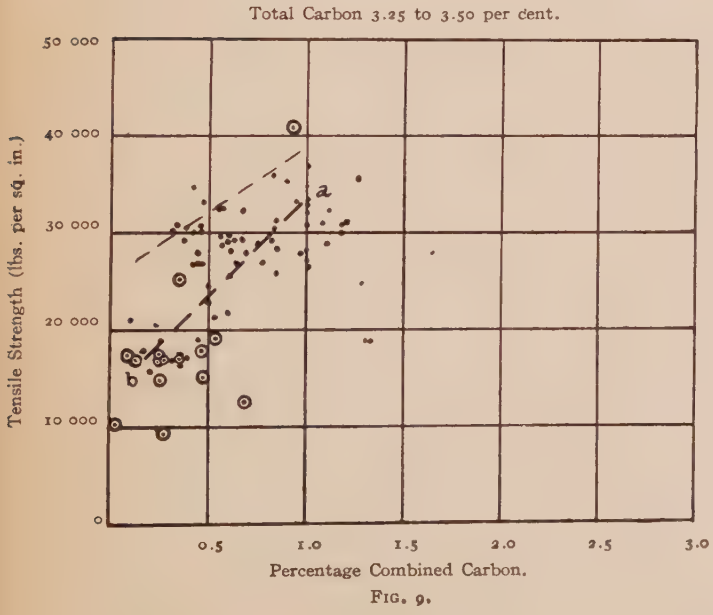



FIG. 9.

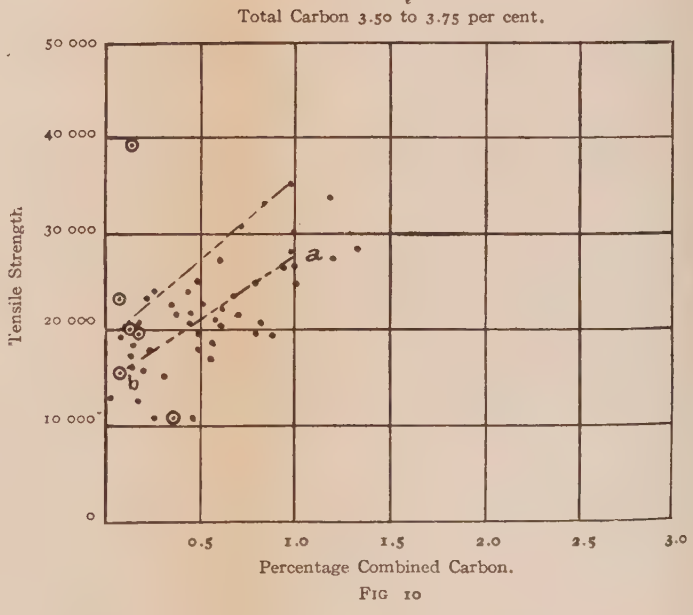



FIG 10

FIGS. 5 to 12. The Relation between Tensile Strength and the Condition of

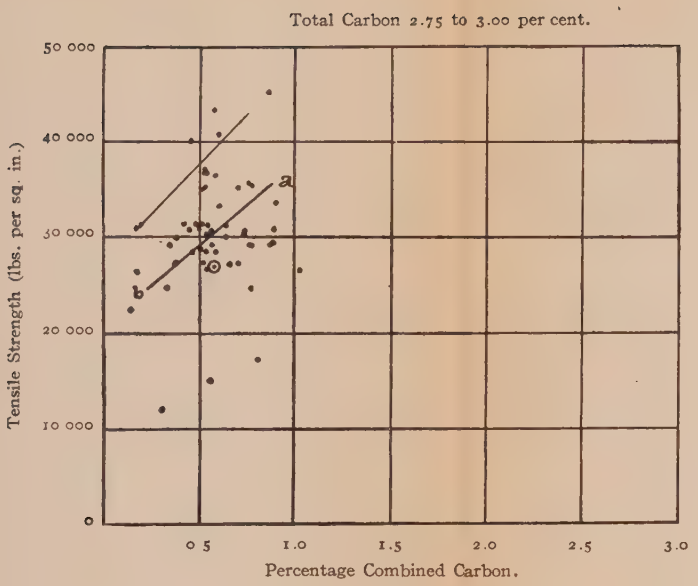



FIG. 7.

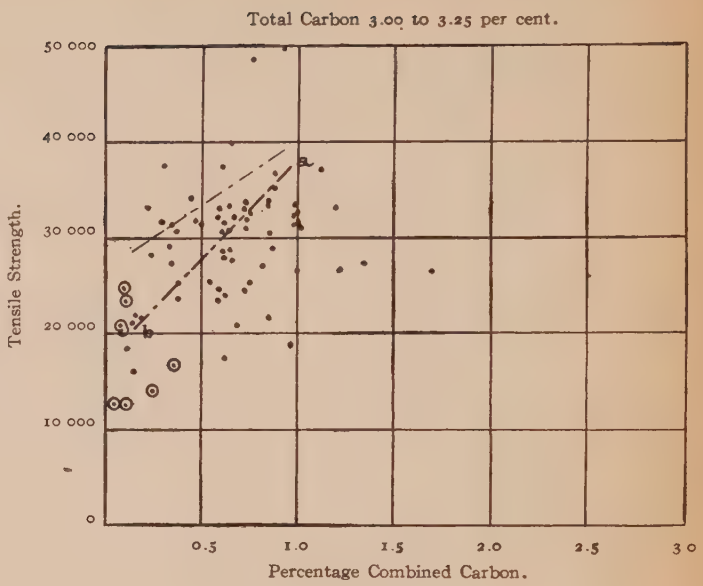



FIG. 8.

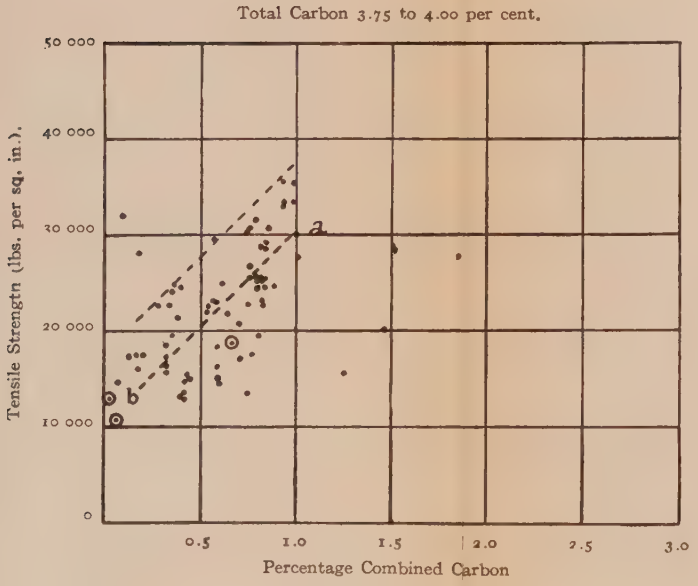



FIG. 11.

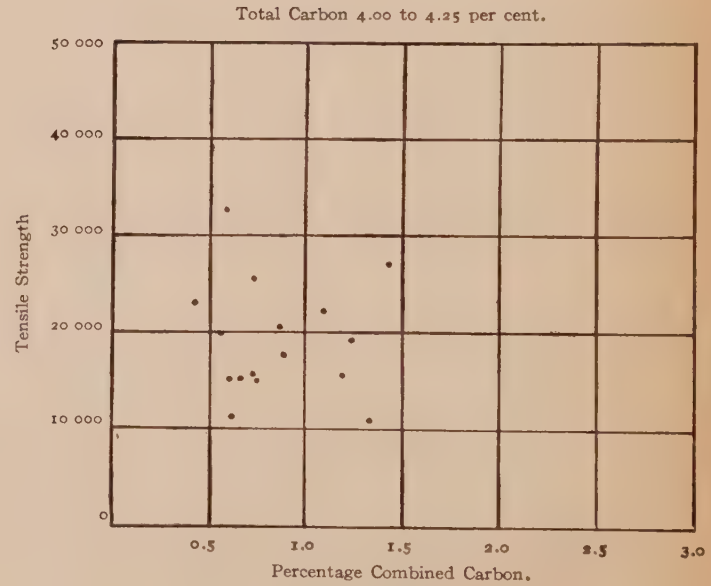



FIG. 12

arbon, for Various Percentages of Total Carbon. (⊙ = Silicon over 2.5 per. cent.)

I further rejected all cast irons which contained either more than 1.00% of phosphorus or more than 0.20% of sulphur. This I did because it seemed to me that in all such cases the influence of the proportion of combined and graphitic carbon would be so greatly masked by the variation in these other features of composition that the data would probably be misleading.

After this rejec[illegible]n. I divided the remaining cases according

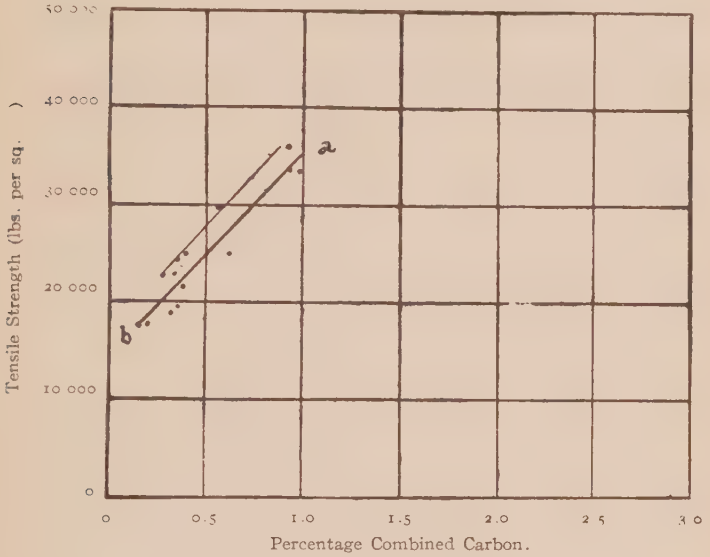



FIG. 13. The Relation between Tensile Strength and the Condition of Carbon in Cast Iron. Series from Data of G. R. Johnson.*

Silicon, Sulphur, and Phosphorus constant, Combined and Graphitic Carbon variable. (Silicon, 1.20 to 1.29; Sulphur, 0.053 to 0.069 Phosphorus, 0.17 to 0.179; total Carbon, 3.76 to 3.93 per cent.)

*Four Iron and Steel Inst., 1898, II, p. 210, Table I.

to their total carbon, into eight groups, so that in each group, except the first, Fig. 5, the total carbon varied only by 0.25%. Then, regarding the total carbon of each group as nearly constant, I proceeded to test corollary 1 by plotting the data of each group separately in Figs. 5 to 12, with the tensile strength as ordinate and combined carbon as abscissa. If corollary 1 is correct, then the highest spots in each figure should be in the

neighborhood of the 1% or the 1.20% carbon ordinate, and the spots should incline downward to right and left of this ordinate.

In those groups which have the largest number of cases there is a fair degree of correspondence with this corollary. It is certainly true that in these groups, Figs. 7 to 11 inclusive, there appears to be a decided rise towards a maximum as the combined carbon increases from 0 to 1%, and a fall as the carbon increases still farther, though the number of cases with more than 1% is so small that this decrease cannot be traced with so much confidence as the increase from 0 to 1% carbon. For the same reason, it is not clear whether the maximum is at 1.20% or at 1%; indeed, the data point to 1% rather than to 1.20% of combined carbon as the maximum point.

It is not pretended that the agreement between this corollary and these data is so close as to support the hypothesis very powerfully. Here, as in our other tests, all that could be hoped was that no glaring disagreement between facts and our hypothesis would be found; and it appears to me that on the whole the agreement is much better than was to be expected.

*Test of Corollary 2.*—The data plotted in Figs. 5 to 12 enable us to test corollary 2 in the following way:

Let us confine ourselves to that part of each group in which the combined carbon is below 1%, because this is the thickly settled part, and let us draw through the axis of each group a line, ab, to show the average tenacity for each percentage of combined carbon. This line, according to corollary 2, should be the higher the smaller is the percentage of graphite, or in other words, the smaller is the total-carbon-content. Such lines have thus been drawn with care, and they have been assembled in Fig. 14, where it will be seen that as a whole they follow this law, thus supporting corollary 2. In like manner a line has been drawn along the upper boundary of each of these groups, and these lines have been assembled in Fig. 15, where, taken as a whole, they obey this law fairly well, thus further supporting corollary 2.

In drawing each line in each group, attention was concentrated wholly on this group, so that the lines were drawn without actual knowledge as to how they would compare when assembled.

In examining Figs. 14 and 15 one point should be borne in mind. It is that the ordinate AB in each of them passes through

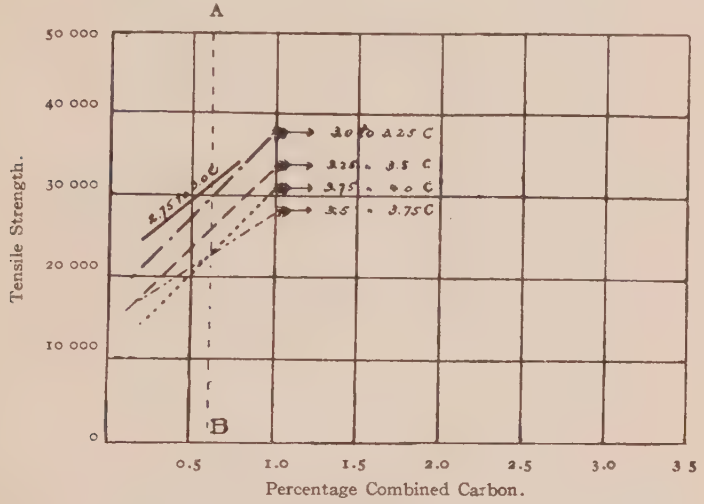



FIG. 14. Axial Lines of Figs. 7 to 11 Assembled. The ordinate A B passes near through the center of all these groups.

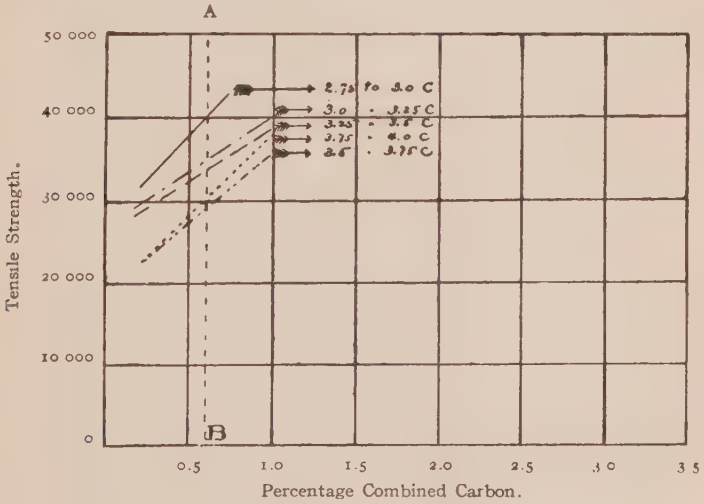



FIG 15. The Upper Boundary Lines of Figs. 7 to 11 Assembled.

or near the center of most of these groups. It gives approximately the average combined carbon of the whole. On this

account the relative position of the several diagonal lines at this ordinate, is particularly important as a test of our hypothesis, and with one exception in each figure these lines follow exactly the order which the hypothesis requires.

The axial and upper boundary lines of Fig. 13 are not reproduced in Figs. 14 and 15 for the sufficient reason that its points have already been included in Fig. 11. These points from Fig. 13 lie higher than the axial line of Fig. 11, and, taken by themselves, rather higher than the hypothesis calls for, *i. e.*, these particular ones taken apart do not agree so closely with the hypothesis as the group, taken as a whole, assembled in Fig. 11 does. The high tensile strength of these irons may be referred to their purity, their freedom from phosphorus and sulphur. On account of this great purity they are not, taken by themselves, strictly comparable with the data in the other figures.

*Estimate of the Quantitative Influence of Graphite on Tensile Strength.*—The assemblage of lines in Fig. 14 enables us to make a rough estimate of the weakening effect of graphite on tensile strength. Comparing any two of these lines, at any given ordinate the percentage of combined carbon is the same for both lines, and hence the constitution of the matrix, and hence in turn its properties, are the same for both lines. The only difference in composition, then, is the percentage of graphite; and therefore we must assume that this difference in graphite-content is the cause of the vertical distance between the lines at such ordinate. In other words, the cast iron of the lower line at such ordinate has more graphite than the cast iron of the upper line at that ordinate, and it is also weaker by the amount represented by the gap between the two lines there; this gap therefore represents the weakening effect of the difference in graphite-content for these two cast irons.

But, comparing different ordinates for these same two lines, the difference in graphite-content at any one ordinate is the same as at any other ordinate.

Hence we infer that the gap between any two lines is due to the difference in graphite-content between those lines.

Let us select the ordinate AB as giving the most trustworthy information, because based on the greatest number of

cases. At this ordinate the vertical distance between the highest and lowest lines (or rather between the highest line and the crossing of the lowest two lines because these here cross each other) is about 8 750 pounds per square inch. Taking the upper line as representing total carbon 2.87% and the junction of the lowest lines as representing carbon 3.75%, the difference in carbon-content is $3.75 - 2.87 = 0.88\%$. But, as we have already seen, since the combined carbon is the same for both lines here, this difference of 0.88% is really a difference in graphite-content of 0.88%. If this is the real cause of the difference of 8 750 pounds per square inch, then the influence of 1% of graphite within these limits is $8\,750 \div 0.88 =$ say 10 000 pounds per square inch.

## SUMMARY.

To sum up the results of this investigation, the way in which the properties of cast iron actually vary with variations in the distribution of the carbon between the combined and graphitic states, as given at the bottom of Fig. 2, is in rough harmony with my hypothesis. Further, two corollaries from my hypothesis, when subjected to test, agree with it as fully as could reasonably be expected.

From these facts I infer that the hypothesis, because of its veridacity and of this preliminary agreement between it and the facts, is worthy of further testing.

## ADDENDUM.

*Addendum, January 27, 1903.*—It is only to-day, after the foregoing has been written and most of it has been set up, and after the foregoing engravings have been made, that I have seen what I ought to have seen before, and probably should have seen but for the pressure under which I have been forced to write much of this article in order to keep my promise to have it in time for the present volume. That to which I refer is the fact that the data at hand, on which Figs. 5 to 15 are based, really enable us to test directly the forecast as to the tensile strength of the three series of cast irons containing respectively 2.00, 4.00 and 4.50% of total carbon represented by line GAF in Fig. 2 and lines LMN and HJK in Fig. 4. The 4.00% carbon line GAF

is susceptible of the most trustworthy test, and this test we will consider first. Then we will consider tests applied to this same line and the 2.00% and 4.50% carbon lines jointly.

*Test of the 4.00% Carbon Line, GAF, from Fig. 2.*—The left-hand part of this line, GA, is reproduced as GA in Fig. 16,

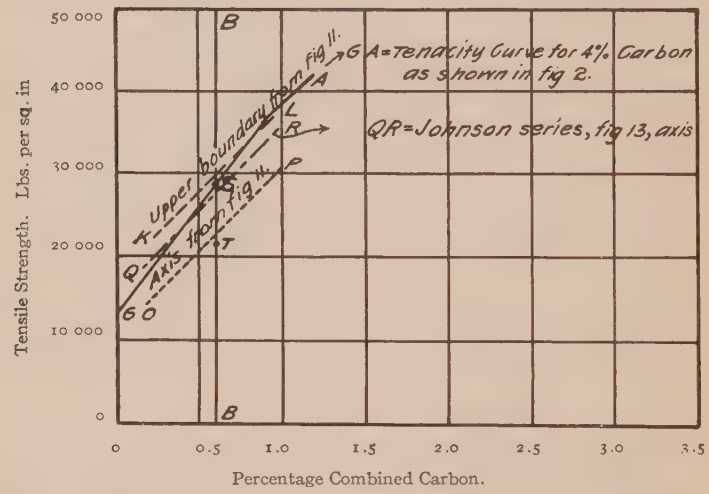



FIG. 16. Comparison of line GAF from Fig. 2, deduced from my hypothesis, with lines from Figs. 11 and 13, directly based on experimental data.

and for comparison the axial and upper boundary lines of the group of data nearest in carbon, with carbon 3.75 to 4.00%, are reproduced from Fig. 11 as OP and KL respectively. On comparing these lines it appears that, within the limits here shown, the tensile strength curve GA which I evolved by a process of reasoning for iron containing 4.00% of carbon, actually lies between the upper boundary and the axis of the points representing the actual tensile strength of such iron, and nearly parallel with them. In other words, the position of this line as inferred from our hypothesis lies very close to its true position as shown by this accumulation of data.

Indeed, it ought to, as it does, lie above the axial line, because its position was deduced from the tensile strength of carbon steels, which are relatively free from the hurtful and probably weakening elements, phosphorus and sulphur, while

the axial line is based on the data of commercial cast irons which doubtless as a whole contained much more of these hurtful elements.

This idea is supported by comparing the Johnson data for pure cast iron, nearly free from phosphorus and sulphur and containing between 3.75 and 4.00% of carbon. To do this I have reproduced the axial line from Fig. 13 as the line QR in Fig. 16. It ought to be closely comparable with line GA as deduced from my hypothesis; and actually it coincides with it almost exactly. Here then my hypothesis receives welcome support.

The tests which have just been described compare the tensile strength line GAF of Fig. 2 with the lines representing the actual strength of cast irons containing from 3.75 to 4.00% of carbon from Figs. 11 and 13. Not content with this comparison, because it is based on a single group of points, that is to say, those within these carbon limits, I wished to compare the position of GAF with the whole of my data in the following way. I proceeded to calculate at what point the axial line for 4.00% of carbon, and the upper boundary line for 4.00% of carbon, should cut the ordinate BB (given in Figs. 14 and 15 as AB) and to see how these points were related to that at which GAF cuts that ordinate. In making this calculation I took into account the vertical distance, measured at ordinate BB, between the line for 3.75 to 4.00% of carbon and each of the other lines (except that for 3.50 to 3.75% of carbon) in Figs. 14 and 15 respectively. Calculated thus, the upper boundary line should cut BB at S; and this is actually where GAF cuts it: and the axial line should cut it at T. In other words, at this ordinate, which represents the thickest dotted part of the field, and is therefore the most trustworthy, the calculated strength of cast iron derived from my hypothesis is practically exactly the same as that indicated as the upper boundary by these accumulated data.

*Test of the* 2.00%, 4.00% and 4.50% *Carbon Lines LMN, GAF,, and HJK from Figs. 2 and* 4.—I next proceeded to compare these three lines with those of Figs. 14 and 15 by plotting both sets directly from these different figures in Figs. 17 and 18. The parts here plotted of GA, LM and HJ of lines

GAF, LMN and HJK may be recognized through bearing here the same letters as in Figs. 2 and 4.

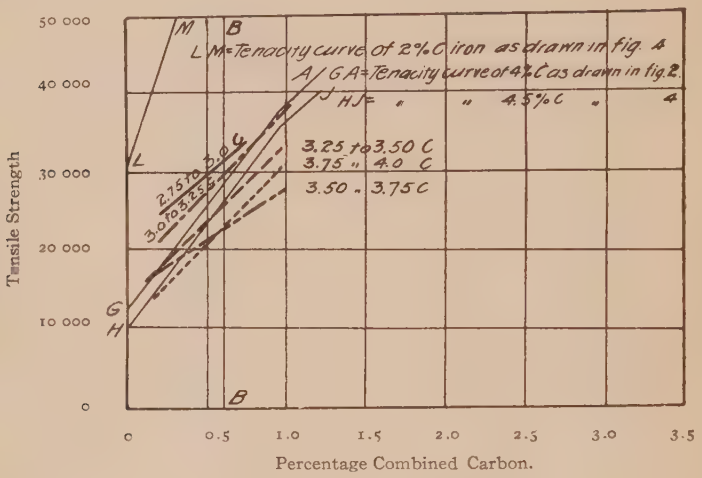



FIG. 17. Axial lines from Fig. 14.

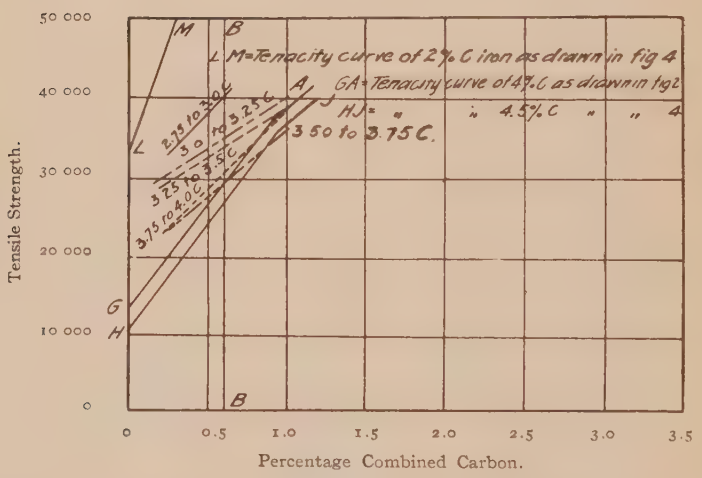



FIG. 18. Upper boundary lines from Fig. 15.

Figs. 17 and 18. Comparison of lines GAF, LMN and HJK, deduced from my hypothesis, with the lines from Figs. 14 and 15, based directly on experimental data.

Comparing the position of lines GA and HJ, for 4.00% and 4.50% of carbon respectively, with the lines from Figs.

14 and 15, we see that each of the former lies rather higher than it should when compared with the axial lines, and rather lower than it should when compared with the upper boundary lines. In other words, the lines deduced from our hypothesis here too are intermediate between the position which our data accord to the axial and that which they accord to the upper boundary line; so that the position of GA and HJ compare with the direct data much in the same way that line GA does when compared with the lines from Fig. 11 alone. This intermediate position is where we should expect to find them. In short, tested thus, these two tenacity lines as derived from our hypothesis agree reasonably with those based on direct data. But while they are roughly parallel, the former lines are somewhat steeper than the latter, indicating that some additional correction must be introduced into our hypothesis to square its deductions accurately with the facts.

The 2.00% carbon line LM, however, is considerably higher than and much steeper than the data in Figs. 14 and 15 indicate that it should be. This, too, indicates the need of some further correction: though it is to be borne in mind (1) that line LMN was not drawn with any attempt at accuracy, but only to show its general shape, (2) that it is so far in composition from what we may call our carefully calculated line GAF, that it cannot be expected to be accurately placed, and (3) that our data plotted in Figs. 14 and 15 differ widely from it in carbon-content. These facts diminish the weight which should be attached to the discrepancy between the sketched position of LMN and the data of Figs. 14 and 15.

## DISCUSSION.

Mr. Sauveur.

ALBERT SAUVEUR.—I am very well acquainted with Professor Howe's theory of the constitution of cast iron, and I should like to say a few words about it. I share it to the fullest extent, and I think the position he has taken is a very logical one. I know that some foundrymen resent the intrusion of steel men whom they consider as laymen in the field of cast iron. They claim cast iron is a metal entirely different from steel, and while conceding that steel has been investigated more thoroughly and on more scientific lines than cast iron, they claim that steel and cast iron have very little if anything in common, and that therefore the knowledge gained in the study of steel is of little or no value in the study of cast iron. I share Professor Howe's objections to such an attitude, for instead of having nothing in common, cast iron and steel stand in the very closest relation. Let those who are inclined to oppose this view consider what is left if the graphitic carbon which may be present in cast iron be removed. If it were possible to pick out—to remove bodily—from cast iron, every particle of graphitic carbon, the residue would be unquestionably a mass of steel, with so many cavities once occupied by graphitic carbon. The character of that mass of steel would of course vary with the grade of the cast iron. If the cast iron was originally perfectly gray, the metal left, after removing the particles of graphite, would be practically wrought iron; and as the combined carbon increases, the residue would be a mass of steel of increasing carbon content. No. 2 foundry iron, for instance, would leave a mass of medium hard steel, if I may so call it; while mottled iron would leave, after removing the graphitic carbon, a mass of very hard steel. Gray cast iron is composed then of a steel matrix, and of a certain number of particles of graphitic carbon; I do not think that we can get away from that fact. Our knowledge of steel, therefore, implies a knowledge of the matrix of cast iron. What we know about steel can be applied directly, and with very important results, to the study of cast iron.

Mr. Sauveur.

To give only one instance of this, let us suppose that we desire to produce a cast iron of the greatest possible tensile strength. Is it not evident that we need then a matrix of the greatest possible tenacity? And here our knowledge of steel is of direct value. We know what kind of steel possesses the greatest tenacity; we know that if we have 0.9 to 1% of carbon we obtain the greatest tenacity; therefore, on logical ground we can say that in order to produce the strongest possible cast iron, the matrix should contain about that amount of carbon. In the case of steel the metal is then said to be saturated. The matrix of the cast iron should likewise be saturated, i.e., it should be a mass of pearlite. Moreover, nobody will deny, I think, that graphitic carbon not only adds nothing to the strength of cast iron, but that it decreases its strength. Therefore, as a second requirement, we should have as little graphitic carbon as possible, which in turn means a small amount of total carbon. I am well aware, however, that the amount of total carbon can be regulated only within narrow limits. In the cupola furnace cast iron will generally take up from 3.25 to 4% of total carbon; which means a considerable amount of graphitic carbon in the casting. It is, therefore, chiefly upon the strength of the matrix that we must depend in order to obtain a casting of great strength. In the so-called "air furnace," however, the amount of carbon can be reduced, and it should be possible to produce stronger castings in such a furnace than in a cupola. The character of the matrix will of course be greatly influenced by the percentage of silicon and by the rate of cooling. This is too well understood to demand more than a passing remark.

I should like in this connection to call your attention to the value of metallography in the study of cast iron. If you accept the view that the strength, and other properties of cast iron depend primarily upon the strength and properties of its matrix, it is of very great importance to know what the matrix is. Since the character of the matrix depends primarily upon the amount of combined carbon, it might be said that it is necessary, in order to ascertain the nature of the matrix, to determine the amount of combined carbon in the cast iron. To do so, however, requires, as we all know, two combustion determinations: one for the total carbon, and one for the graphitic carbon. The sub-

Mr. Sauveur. traction of these two results gives us the amount of combined carbon. This is a long determination. Moreover, the determination of graphitic carbon is, I think, not a very satisfactory one; and after we have subtracted those figures supposed to represent graphitic carbon from the total carbon, there must remain in our mind some uncertainty as to the resulting figures representing accurately the amount of combined carbon. The analytical method gives us but an uncertain idea as to the exact character of the matrix. On the other hand, if we call metallography to our assistance, it will take but some fifteen minutes to polish properly a chip of cast iron, place it under the microscope, and observe its structure. The advantage of looking at the thing itself, of seeing before you the matrix of the iron, of ascertaining whether it is saturated or not—is a decided one. In this and in similar directions metallographic methods should prove of much value to the foundryman.

Mr. Moldenke. R. G. MOLDENKE.—I should like to add that I also agree perfectly with Professor Howe on his theory concerning the constitution of cast iron. I myself have been working on that same theory twelve years. As long ago as that I recognized that cast iron is nothing more than impure steel with a lot of graphite in crystals interspersed and that if that steel portion of the casting could be refined it would lead to better results.

Mr. Howe. HENRY M. HOWE (by letter).—It is very gratifying to find that such competent authorities as Messrs. Sauveur and Moldenke agree with me as to the general features of the matter. It would be superfluous for me to say anything here on the subject for the reason that the paper which I have now sent in to the Secretary gives my views so very fully.

# APPENDIX I.

## REPORT ON THE BUDA-PESTH CONGRESS

OF THE

## INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS,

SEPTEMBER 9 TO 14, 1901.

BY HENRY M. HOWE,
Member of Council. Chairman of the American Section.

[Presented to the Executive Committee of the American Section, November 2, 1901.]

The essential matters which I was directed to attend to at the Buda-Pesth Congress were the following:

1. The recognition of the American Section.
2. The appointment of a Committee on Cast Iron, particularly to report on standard reception specifications.
3. The appointment of a like Committee on Cement.
4. The publication of papers in English.
5. The election of the American Member of Council by the American Section.

I beg leave to report my action as follows:

RECOGNITION OF SECTION.—I found the opposition to this so firm that it appeared useless to press the matter. The reason for this opposition I did not very clearly make out, unless it be that in some European countries there is a large minority of the members who prefer not to join the quasi-branch organizations in those countries. Therefore those organizations are not recognized as direct branches or sections, but as independent organizations. The Council urged upon me the point of view that if our section joined the association as a single member, we should have every possible liberty that we could have if we retained the name of section. On account of the feeling which

some of us have that if we followed this course we might in some way lose our powers or privileges, I presented a resolution to the following effect:

"*Resolution.*—Members of the International Association for Testing Materials who are at the same time members of a society, which society is itself a member of the International Association, retain all the rights and privileges which they would have if they were not members of such a society."

It was held that this was self-evident; nevertheless at my urgent request the resolution was adopted unanimously. My suggestion is that the section change its title and become a member of the Association. In elections of members of Council it would be necessary that all members of the Association resident in America, even those who are not members of the American Section, shall be entitled to vote.

PUBLICATION OF PAPERS IN THE ENGLISH LANGUAGE.—The action taken by the Society in this matter seems to me ample. Great difficulties were arising at the time of the Congress, in negotiating arrangements with the owner of *Baumaterialienkunde*, and after careful consideration I deemed it unwise to complicate the matter by a formal resolution.

STANDARD SPECIFICATIONS.—I found at first serious opposition on the part of many members to even considering the idea of erecting standard specifications. Through a confusion it has got into their heads that these specifications would, if made standard, also be made obligatory, if not immediately, then later on. In the case of cement the opposition was very much greater than in case of cast iron, and the cement manufacturers and those interested in testing cement seem to have serious differences. Apart from these active opponents of the erection of standard specifications, the general feeling among the members of the Congress seemed to be that the first duty of the Association was to agree on methods of testing, and that while the setting up of standard or normal specifications was in itself a good thing, the other question had the right of way. I endeavored to impress upon them that this was not the view taken in this country, and that it appeared to us, so far as I could judge, that the setting up of standard specifications was the field of greatest usefulness of the Association.

The objection to having the committees take up the question of standard specifications was so strong that at first I feared that it would be impossible to bring it about. But with the very efficient aid of Mr. Walter Wood and Dr. R. Moldenke I induced the Congress to take the following steps:

1. Committee No. 1 was directed to consider the question of the framing of standard specifications and if possible to report such specifications to the St. Petersburg Congress of 1903.*

2. The proposition to appoint a new committee to report on standard specifications and methods of testing cast iron and finished castings was referred to the Council with power.

3. A new committee was appointed for the methods of testing cast iron, as distinguished from the reception tests of cast iron.

4. Committee No. 22 was charged with considering the feasibility of the setting up of standard international specifications for cements and mortars, and it was directed to report such specifications to the St. Petersburg Congress if possible.

ELECTION OF MEMBER OF COUNCIL.—The draft of statutes submitted to the members before the Congress provided, as you will remember, that the members of Council were to be chosen by members resident in each country entitled to this representation, subject to confirmation by the Congress, and were to last from Congress to Congress. I pointed out how awkward the situation would be if the Congress refused to ratify, for in that case the country would have to go unrepresented, because it would be impossible to hold a new election in time for ratification by that same Congress, and no new Congress would convene until after the space of two years. I therefore moved that the provision requiring the ratification by the Congress be struck out, making the election by the members of each country final and absolute. This was unanimously adopted.

AMERICAN MEMBER OF COUNCIL.—It was held that the election which took place about a year ago in this country, was to be regarded as an election covering the period from the Congress of 1901 to the Congress of 1903, and I was accordingly enrolled

* Since this report was written it has been decided to hold the St. Petersburg Congress in 1894.

as Member of Council for the present term. I pointed out, however, that as I had already served in effect nearly two years, and that as American custom looked toward rotation in office, it was to be expected that another member of Council would be chosen in my place.

IN GENERAL.—The whole tone of President Tetmajer and indeed of all the Members of Council was in the highest degree friendly and conciliatory, and it was evident that there was every wish to place our relations on the most friendly basis, and that to remove all traces of former friction. As evidence of this I may mention that I, as your representative, was chosen Honorary President of the Congress as well as Chairman of the most important Section—that of metals. The Association showed great vitality, as may be inferred from the number of very important papers presented.

Respectfully submitted,

HENRY M. HOWE,
Member of Council.

# APPENDIX II.

## BIBLIOGRAPHY ON IMPACT TESTS AND IMPACT TESTING MACHINES.

By W. Kendrick Hatt and Edgar Marburg,
Committee on the Present State of Knowledge Concerning Impact Tests.

### Introduction.

A preliminary report on the present state of knowledge concerning impact tests was prepared by this Committee and presented to the Society—known then as the American Section of the International Association for Testing Materials—at the second annual meeting, held in Pittsburg in 1899. This report, published as Bulletin No. 5 of the Proceedings, contains a brief discussion of the subject of impact tests, including experimental data; a consideration of the requirements to be met by impact testing machines; a review of American and German practice in commercial impact tests on cast iron, steel rails, car wheels and car couplers; concluding with a synopsis of the views of American testing experts on the practical value of impact tests.

The present and final report of the Committee is devoted wholly to a presentation of the bibliography on impact tests and testing machines.

The references to English sources are believed to include all important contributions to the subject up to July 1, 1902, as well as numerous minor articles, in part of little more than historic interest. The references to German and French sources are, for obvious reasons, less complete. In that connection the undersigned desire to make their appreciative acknowledgment of the courteous assistance of Professor A. Martens, in supplying valuable references to foreign literature on the subject, especially in German.

For convenience of reference the English and French titles are grouped under three headings, according to the nature of the contents. The German literature referred to was in part

inaccessible to the authors, and no attempt was, therefore, made at classification. The references to text-book matter do not aim at completeness; but are limited to a few standard treatises containing the ground-work of the subject. Reprints, partial or complete, of original papers are, as a rule, not indexed. A few exceptions are made for special reasons, for example, where the sources themselves are not readily accessible. The sources quoted are arranged in alphabetic order throughout. In the case of several titles from the same source, they are arranged in the sequence of the dates of publication.

The articles indexed were, for the most part, examined independently by the members of the Committee. In a few cases the literature was unavailable, and the references could not be verified. In a few instances the titles could not be cited in the original language and appear only in the translated form obtained at secondhand.

The revival of interest in impact tests, especially in Europe, is well evidenced by the presentation of four important contributions to the subject at the last Congress of the International Association for Testing Materials, held in Buda-Pesth in 1901. It was decided at this Congress to appoint a special committee to report on "Tests with notched bars for ascertaining the relations between the different methods of testing and for fixing the different properties of metals." In America, too, this branch of testing is receiving an increasing measure of attention and new forms of apparatus have been successfully developed within recent years. It is hoped that the bibliography herewith presented may prove helpful to those interested in the subject.

Respectfully submitted,

W. KENDRICK HATT,
EDGAR MARBURG.

## REFERENCE LIST—TITLES AND ABBREVIATIONS.

### (*a*) IN ENGLISH.

**Am. Assoc. Adv. Sc.**—Proceedings of the American Association for the Advancement of Science, Washington, D. C.

**Am. Inst. Min. Eng.**—Transactions of the American Institute of Mining Engineers, New York, N. Y.

**Am. Sect. Int. Assoc. Test'g Mat's.**—Bulletin American Section of International Association for Testing Materials (now the American Society for Testing Materials), Philadelphia, Pa.

**Am. Soc. Civ. Eng.**—Transactions of the American Society of Civil Engineers, New York, N. Y.

**Am. Soc. Mech. Eng.**—Transactions of the American Society of Mechanical Engineers, New York, N. Y.

**Assoc. Eng. Cornell Univ.**—Transactions of the Association of Engineers of Cornell University, Ithaca, N. Y.

**Dig. Phys. Tests.**—Digest of Physical Tests, Philadelphia, Pa.

**Engin'g.**—Engineering, London.

**Eng. News.**—Engineering News, New York, N. Y.

**Eng. Rec.**—Engineering Record, New York, N. Y.

**Eng. Soc. West. Pa.**—Proceedings of the Engineering Society of Western Pennsylvania, Pittsburg, Pa.

**Inst. Civ. Eng.**—Proceedings of the Institution of Civil Engineers, London.

**Int. Assoc. Test'g Mat's.**—Proceedings of the International Association for Testing Materials, Vienna, Austria.

**Jour. Fr. Inst.**—Journal of the Franklin Institute, Philadelphia, Pa.

**Mass. Highw. Comm. R'p't.**—Massachusetts Highway Commissioners' Report, Boston, Mass.

**M. C. B. Assoc.**—Proceedings of the Master Car Builders' Association, Chicago, Ill.

**R. R. Gaz.**—Railroad Gazette, New York, N. Y.

**Techn.**—The Technograph, University of Illinois, Champaign, Ill.

**Tests of Met's.**—Tests of Metals and Other Materials, Watertown Arsenal, Watertown, Mass.

**The Eng.**—The Engineer, London.

### (*b*) IN FRENCH.

**Assoc. Ingen. Ponts et Chauss.**—Associacion des Ingénieurs des Ponts et Chaussées du Sud Ouest, Bordeaux, France.

**Comm. Meth. d'Essai.**—Commission des Méthodes d'Essai des Matériaux de Construction. Première Session. J. Rothschild, Paris, 1894.

**Comptes Rendus.**—Comptes Rendus de l'Académie des Sciences, Paris.

**Congr. Int. Meth. d'Essai.**—Communications présentées devant le Congrés International des Méthodes d'Essai des Matériaux de Construction, 1900. Dunod, Paris, 1901.

**Fond. d. Can. d. Bourges.**—Fonderie des Canons des Bourges, Bourges, France.

**Genie Civ.**—Génie Civil, Paris.

**Jour. de Phys. Th. et App.**—Journal de Physique, Théorique et Appliquée, Paris.

**Lab. d'Ec. Nation. d. Ponts et Chauss.**—Laboratoire d'Ecole Nationale des Ponts et Chaussées, Paris.

**Manuf. d'Armes de St. Et.**—Manufacture d'Armes de St. Etienne, St. Etienne, France.

**Manuf. d'Armes de Chat.**—Manufacture d'Armes de Châtellerault, Châtellerault, France.

**Rev. d'Artill.**—Revue d'Artillerie, Paris, France.

**Soc. d'Encour.**—Bulletin de la Société d'Encouragement, Paris.

**Soc. Ing. Civ. de Fr.**—Bulletin de la Société des Ingénieurs Civils de France, Paris, France.

### (*c*) IN GERMAN.

**Bamkd.**—Baumaterialienkunde, Stuttgart, Germany.

**Chem. Zeit'g.**—Chemiker Zeitung, Cöthen, Germany.

**Der Civiling.**—Der Civilingenieur, Leipsic, Germany.

**Mitth. Berlin.**—Mittheilungen aus den Königl. technischen Versuchsanstalten zu Berlin, Berlin.

**Mitth. Munchen.**—Mittheilungen des mechanisch-technischen Laboratoriums der Königl. technischen Hochschule, Munich, Germany.

**Mitth. Wien.**—Mittheilungen des K. K. technologischen Gewerbemuseums, Vienna.

**Mitth. Zurich.**—Mittheilungen des Eidgen. Polytechnikums, Zurich, Switzerland.

**Stahl und Eisen.**—Stahl und Eisen, Düsseldorf, Germany.

**Zeitschr. Ver. Deutsch. Ing.**—Zeitschrift des Vereines deutscher Ingenieure, Berlin.

**Zeitschr. Berg. Hutt. u. Sal.**—Zeitschrift für Berg-, Hütten-, und Salinenwesen, Berlin.

# I. ORIGINAL DATA: TESTS—DESCRIPTIONS OF NEW MACHINES, ETC.

(*a*) IN ENGLISH.

**Am. Inst. Min. Eng.**—Trans., Vol. 8, 1880, pp. 76–78.
Apparatus for Testing the Resistance of Metal to Repeated Shocks.—Wm. Kent.
Description of an apparatus for producing a succession of longitudinal shocks.

**Am. Inst. Min. Eng.**—Trans., Vol. 18, 1890, pp. 798–835.
Test of Metals. Discussion (p. 805).—W. J. Keep.
Description of W. J. Keep's impact machine, with results of tests.

**Am. Inst. Min. Eng.**—Trans., Vol. 26, 1896, pp. 176–185.
The Mobility of Molecules of Cast Iron.—A. E. Outerbridge.
See also Jour. Fr. Inst. Vol. 142, 1896, pp. 69–72, p. 149.

**Am. Soc. Civ. Eng.**—Trans., Vol. 5, 1876, pp. 199–214.
Note on the Resistance of Metals as Affected by Flow, and by Rapidity of Distortion.—R. H. Thurston.
See also Dingler's Journal, February, 1877, p. 333.

**Am. Soc. Civ. Eng.**—Trans., Vol. 32, 1894, pp. 536–539.
The Strength and Weathering Qualities of Roofing Slates.—Mansfield Merriman.

**Am. Soc. Civ. Eng.**—Trans., Vol. 33, 1895, pp. 297–343.
Tests of Materials for Metallic Structures. Discussion (pp. 305–307).
Specifications for Structural Steel.—H. H. Campbell.
Tests of bars of structural steel containing 1 per cent. manganese, hammered while under tension.

**Am. Soc. Civ. Eng.**—Trans., Vol. 39, 1898, pp. 237–273.
Experiments with a New Machine for Testing Materials by Impact.—S. Bent Russell.
Describes pendulum machine and transverse tests of notched bars of various materials.
See also Eng. News, Vol. 39, 1898, p. 30.

**Am. Soc. Civ. Eng.**—Trans., Vol. 43, 1900, pp. 1–16.
Impact Tests of Structural Steel.—S. Bent Russell.
Describes methods and results of impact tests in tension on notched bars of iron and steel.
See also Eng. News, Vol. 42, 1899, p. 321.

**Am. Soc. Mech. Eng.**—Trans., Vol. 19, 1897, pp. 351–386.
Cast Iron Under Impact.—W. J. Keep.
See also Eng. News, Vol. 38, 1897, p. 382.

**Am. Soc. Mech. Eng.**—Trans., Vol. 21, 1899, pp. 369–395.
Impact.—W. J. Keep.

The influence of size and span of cast iron test bar on results of impact test; recommends that bar for determination of resilience be supported at ends, and struck at center; that for detection of brittleness transverse blow be delivered on projecting end of bar. Size of bar, 1″ x 1″ x 24″; weight of hammer, 50 lbs. Rupture by a number of blows of pendulum hammer.

**Eng. News.**—Vol. 45, 1901, p. 14.
Results of Impact Tests in Tension.—S. Bent Russell.

**Eng. News.**—Vol. 45, 1901, p. 82.
Impact Tests at Purdue University.—W. K. Hatt.
See also Eng. News, Vol. 45, 1901, p. 103.

**Eng. Soc. West. Pa.**—Proc., Vol. 3, 1887.
The Effect of Temperature upon Structural Steel.—Jos. Ramsey.
See also Eng. News, Vol. 18, 1887, p. 398.

**Eng. Soc. West. Pa.**—Proc., Vol. 9, 1893, pp. 14–41.
Effect of Suddenly Applied Loads upon the Tensile Strength and Other Physical Properties of Iron and Steel.—E. D. Estrada.
See also Eng. News, Vol. 32, p. 93.

**Inst. Civ. Eng.**—Proc., Vol. 60, 1880, pp. 182–186.
Iron and Steel at Low Temperatures.—J. J. Webster.
Results of tests in transverse impact on cast iron, cast steel and wrought iron at low temperatures.

**Inst. Civ. Eng.**—Proc., Vol. 76, 1883, p. 104.
Discussion.—E. Maitland.
Describes impact experiments in tension under drop, and explosive action of gunpowder in discussion of paper on "Forms of Test Pieces," by W. Hackney.

**Inst. Civ. Eng.**—Proc., Vols. 87, 94, 105. Part I, Vol. 87, 1887, pp. 340–370; Part II, Vol. 94, 1888, pp. 180–208; Part III, Vol. 105, 1891, pp. 161–176.
Effect of Temperature on the Strength of Railway Axles.—Thomas Andrews.
See also Eng. News, Vol. 19, 1888, pp. 112, 130. Eng. Rec. Vol. 32, 1895, p. 109.

**Inst. Civ. Eng.**—Proc., Vol. 95, 1889, pp. 354–358.
On the Use of Heavier Rails for Safety and Economy in Railway Traffic.—O. P. Sandberg.
Account of drop tests on rails at low temperatures.

**Inst. Civ. Eng.**—Proc., Vol. 103, 1891, pp. 231–249.
Effect of Chilling on the Impact Resistance of Metals.—Thomas Andrews.

**Iron and Steel.**—Knut Styffe.
English Translation by C. P. Sandberg, p. 151.
Appendix: Tests by C. P. Sandberg on rails at low temperatures.
See also Inst. C. E., Proc., Vol. 60, 1879, p. 164.

**Jour. Fr. Inst.**—Vol. 144, 1897, p. 321; Vol. 145, 1898, p. 36–47.
A Photographic Impact Testing Machine.—B. W. Dunn.

The compression of a copper cylinder under a falling weight, and the resulting reaction, were measured by means of a very delicate apparatus.

**Mass. Highw. Comm. Rpt.**—1900. Pp. 69, 71, 81.

Impact and Cementation Tests on Road Materials.—L. W. Page.

**Tests of Met's.**—Vol. 1, 1881, pp. 122–146, 212–222.

Results of impact tests of various grades of wrought iron with photographs of fractures.

**Techn.**—No. 12, 1898.

Impact of Beams.—O. C. Wray.

Abstract of thesis.

(*b*) IN FRENCH.

**Assoc. Ingen. Ponts et Chauss.**—1888.

Resistance au Choc et Fragilité du Fer. (Resistance to Impact and Brittleness of Iron).—M. Considère.

Impact tension tests of iron wire.

See also Comm. Meth. d'Essai., Vol. 2, p. 344.

**Comm. Meth. d'Essai.**—Vol. 3, pp. 303–310.

Sur la Fragilité après Ecrouissage à Froid (or Brittleness after cold-hammering)—M. Considère.

**Comm. Meth. d'Essai.**—Vol. 3, pp. 311–358.

Sur les Essais Divèrs a Froid. (On Various Cold Tests.)—L. Baclé.

Results of tests in transverse impact on punched bars, annealed and normal. Pp. 319, 356, 357.

See also Materials of Construction J. B. Johnson. Pp. 404.

**Comptes Rendus.**—Vol. 94, 1882, p. 773.

Note sur l'Action de Déformation du Choc Comparée a celle d'un effort continue. (Note on the Deforming Action of Impact compared to that of a Load Gradually Applied.)—M. Marchal.

**Congr. Int. Meth. d'Essai.**—Paris, 1900. Vol. 1, pp. 235–299.

Etude sur les Propriétés Physiques et Mécaniques des Métaux. (Study of the Physical and Mechanical Properties of Metals.)—P. Charbonnier et Ch. Galy-Aché. Tension and compression under impact (p. 275).

**Congr. Int. Meth. d'Essai.**—Paris, 1900. Vol. 1, pp. 351–454.

Evolution des Méthodes et des Appareils. (Evolution of Methods and Apparatus.)—Ch. Frémont.

Pp. 435 et seq.—Proper tests to determine brittleness.

**Congr. Int. Meth. d'Essai.**—Paris, 1900. Vol. 1, pp. 507–521.

Les Nouveaux Appareils dans le Laboratoire d'Essais de l'Université Purdue. (New Apparatus in the Purdue University Laboratory for Testing Materials.)—W. K. Hatt and W. P. Turner.

Describes new type of impact machine.

See also Eng. News, Vol. 45, 1901, p. 3.

**Congr. Int. Meth. d'Essai.**—Paris, 1900. Vol. 2, pp. 1–25.

L'Influence du Temps et de la Température. (Influence of Duration and of Temperature.)—A. Le Chatelier.

**Fond. d. Can. d. Bourges.**

Appareils pour l'Essais au Choc. (Apparatus for Impact Tests.)—Bourges, 1885–1891.

**Genie Civ.**—Vol. 39, 1901, pp. 254–258.

Sur la Fragilité de l'Acier. (On the Brittleness of Steel.)—A. Pourcel.

A review of M. Frémont's experiments on notched bars.

**Genie Civ.**—Vol. 39, 1901, pp. 351–354.

Essais des Métaux par Choc sur Barreaux Entaillées. (Impact Test of Metals on Notched Bars.)—A. Dutreux.

**Int. Assoc. Test'g Mat's.**—Buda-Pesth Congress, 1901.

Essais de Fragilité au Choc sur Barreaux Entaillés. (Tests on the Brittleness of Notched Bars under Impact.)—H. Le Chatelier.

**Int. Assoc. Test'g Mat's.**—Buda-Pesth Congress, 1901.

(Memorandum on the Testing of Metals by means of Notched Bars Bent under the Application of Shocks.)—G. Charpy.

**Jour. de Phys. Th. et App.**—1886, p. 291.

Recherches Expérimentales sur le Choc de Corps Elastiques. (Experimental Researches on Impact of Elastic Bodies.)—H. Schneebeli.

**Lab. d' Ec. Nation. d. Ponts et Chauss.**—P. 17, Plate V.

Sonnette pour Essais de Choc.

Describes machine for compression impact test on road material.

See also Cong. Int. Meth. d'Essai, Vol. 1, p. 472.

**Manuf. d'Armes de St. Et.**—1890.

Note sur les Essais au Choc avec des Barreaux Irregulaires. (Note on Impact Tests with Irregular Bars.)

**Manuf. d'Armes de Chat.**—1891.

Note sur quelques Essais de Choc Exécutes a la Manufacture. (Note on some Impact Tests made at the Factory.)

**Rev. d'Artill.**—September, 1890, p. 485.

Expériences sur les Propriétés de l'Acier aux Tres Basses Températures. (Experiments on the Properties of Steel at very low Temperatures.)—C. Bernadou.

Impact tests in flexure on steel.

See also Soc. d'Encour., Vol. 5, No. 8, 1899, p. 1182.—Comm. Meth. d'Essai, Vol. 2, p. 325.—Inst. C. E., Vol. 103, 1891, pp. 478–480.

**Soc. d'Encour.**—Vol. 4, No. 8, August, 1899.

Influence de la Température sur les Propriétés Resistantes des Métaux. (Influence of Temperature on the Resistance of Metals.)—M. A. Ledebur.

A translation by M. S. Jordan. General history of tests at different temperatures. For shock tests, see pp. 1182, 1186, 1188, 1193.

**Soc. d'Enc.**—Vol. 6, 1901, pp. 254–270.

Etude Expérimentale des Causes de la Fragilité de l'Acier. (Experimental Study of the Causes of Brittleness in Steel.)—Ch. Frémont.

Bending tests on small ruled bars.

**Soc. d'Enc.**—Vol. 6, 1901, pp. 365–389.

Essai des Metaux par Pliage de Barrettes Entaillées. (A Test of Metals by Bending Notched Bars.)—Ch. Frémont.

**Soc. d'Enc.**—Vol. 6, 1901, pp. 505–548.

Etude Expérimentale sur le Pliage des Barrettes Entaillées. (Experimental Study of Bending Test on Notched Bars.)—Frémont et Osmond.

Tests of notched bars according to the system of M. Frémont.

**Soc. Ing. Civ. de Fr.**—April, 1901, pp. 563–575.

Quelques Experiences de Flexion par Choc. (Experiments in Impact in Flexure.)—J. Barba.

Impact tests in flexure on notched steel bars.

**Soc. Ing. Civ. de Fr.**—June, 1901.

L'Essai des Metaux a le Flexion par Choc. (Test of Metals by Impact in Flexure.)—G. Charpy.

Tests of notched bars by pendulum machine.

## II. GENERAL DISCUSSIONS—REPORTS OF COMMITTEES—ADDRESSES, ETC. IN ENGLISH AND FRENCH.

### (*a*) IN ENGLISH.

**Am. Assoc. Adv. Sc.**—Proc., Vol. 43, 1894.

The Resistance of Materials under Impact.—Mansfield Merriman.

The history of the science of testing materials; general considerations of resilience.

See also Eng. News, Vol. 32, 1894, pp. 125–132.

**Am. Inst. Min. Eng.**—Trans., Vol. 29, 1899, pp. 569–648.

Nickel Steel.—David H. Browne.

Drop tests on axles: nickel steel compared with ordinary steel. Communication from Carnegie Steel Co. Gives physical, chemical and drop tests.—P. 595.

**Am. Sect. Int. Assoc. Test'g Mat's.**—Bulletin No. 5, October, 1899.

Report of Committee on Present State of Knowledge Concerning Impact Tests.—W. K. Hatt and Edgar Marburg.

**Am. Soc. Mech. Eng.**—Trans., Vol. 11, 1890, pp. 535–540.

Appendix to Report of Committee on Standard Tests and Methods of Testing Materials.

Report of Resolutions passed at Munich and Dresden Conferences.

**Applied Mechanics.**—J. H. Cotterill. MacMillan, 1895.

Chap. XVI.—Impact and Vibration: General Equation of Impact; Augmentation of Stress by Impact in a Perfectly Elastic Material; Sudden Application of a Load; Impact at High Velocities; Effect of Inertia; Resilience. Chapter XVIII, Art. 232—Impact.

**Applied Mechanics.**—John Perry. Van Nostrand, 898. P. 495 et seq.

Fundamental Considerations; Local Effect of Blows.

**Handbook of Testing Materials.**—A. Martens. Translated by G. C. Henning. Wiley & Sons, 1899. Part 1, p. 203–242.

Resistance to Impact; Detailed Descriptions of Machines and Tests; Influence of Various Factors on Results.

**M. C. B. Assoc.**—Proc.. Vol. 33, 1899, p. 165.
Report of Committee on Car Coupler Tests.
See also Ibid., Vol. 34, 1900.

**M. C. B. Assoc.**—Proc., Vol. 33, 1899, p. 254.
Report of Committee on Wheels and Axles. Specifications for Drop Tests.

**Materials of Construction.**—J. B. Johnson. Wiley & Sons, 1897.
Chap. VI: The Resilience of Materials. Chap. XVIII: Impact and Hardness Tests.

**Mechanics of Materials.**—Mansfield Merriman. Wiley & Sons, 1895.
Chap. IX: The Resilience of Materials; Sudden Loads and Impact; Modulus of Resilience; External Work and Resilience; Elastic Resilience of Beams; Ultimate Resilience; Early History of Resilience; Modern Experiments; Paradoxes of Resilience. Chap. X: Longitudinal Impact. Chap. XI: Static and Sudden Deflections; Deflection under Impact; Pressure Due to Impact; Live Load Velocity. Chap. XII: Resilience under Shear; Resilience under Torsion. Appendix: Velocity of Stress; Vibrations of a Beam.

**Strength of Materials.**—Thos. Box. London, E. & F. Spon, 1883. Pp. 411–448.
General Principles; Resilience of Beams; Effect of Inertia of Beam; Impact on Cast Iron Beams; Effect of a Load on a Beam; Impact out of Center; Resistance to Impact as Deflection Squared; Effects of Inertia not Constant; Resistance as Weight of Beam Simply; Effect of Depth and Breadth; Stiffness a Source of Weakness; Summary of Remarkable Laws; Rolling Load at High Velocities; Effect of Inertia.

**Theory of Structures and Strength of Materials.**—H. T. Bovey. Wiley & Sons, 1896. Pp. 184–198.
Impact: On the Extension of a Prismatic Bar; on the Oscillatory Motion of a Weight at the End of a Vertical Elastic Rod.

(*b*) IN FRENCH

**Comm. Meth. d'Essai.**—Vol. 1, p. 147–155.
Essais de Flexion par Choc. (Impact Flexure Tests.)—General Report.

**Comm. Meth. d'Essai.**—Vol. 2.
Sur les Machines a Essayer les Métaux; Essais au Choc. (On Machines for Testing Metals; Impact Tests.)—General Report.
Machines, p. 369; Propositions, p. 374.

**Comm. Meth. d'Essais.**—Vol. 3, pp. 221–261.
Sur les Essais par le Choc. (On Impact Tests.)—M. Clevault.
Need and usefulness of impact tests; methods and machines.

**Traite Practique de la Machine Locomotive.**—W. Demoulin. Baudry, 1898. Vol. 3, chap. 5, 7.
Specifications for material used on French railroads, with impact tests.

## III. MINOR DISCUSSIONS—EDITORIALS, ETC.

IN ENGLISH.

**Am. Inst. Min. Eng.**—Trans., Vol. 9, 1880, p. 542.
Discussion on Steel Rails —Wm. Sellers.
Remarks on drop test for rails.

**Am. Inst. Min. Eng.**—Trans., Vol. 23, 1893, p. 176.
Drop tests of Forged Manganese Steel.—R. A. Hadfield.
Cites results of tests.

**Am. Soc. Civ. Eng.**—Trans., Vol. 13, 1884, pp. 289.
Discussion on Structural Steel and Iron.—Jas. Christie.
Comparative behavior of steel and iron under drop test.

**Am. Soc. Mech. Eng.**—Trans., Vol. 7, 1885, p. 260.
Effect of Interruption of Drop Test on Iron Axles.
Discussion by Mr. Sanderson of Paper on Crystallization of Wrought Iron by Wm. Hill.

**Assoc. Eng. Cornell Univ.**—Trans., Vol. 6, 1898, pp. 76–79.
Impact Tests on Wire Rope.—Charles F. Hamilton.
Abstract of thesis investigation.

**Dig. Phys. Tests.**—Vol. 2, 1897.
Methods of Testing Materials of Construction.—O. M. Carter.
Analysis of resolutions of conventions at Munich and Dresden, and recommendations of American Society of Mechanical Engineers and French Commission: A translation of a report by M. Baclé to the French Government.
See also Eng. Rec., Vol. 34, p. 175 et seq., and Johnson's Materials of Construction. Appendix C.

**Engin'g.**—Vol. 68, 1899, pp. 633–634.
Bridge Design.—Editorial.
Stresses in bridges due to impact.

**Engin'g.**—Vol. 69, 1900, p. 263.
Letter to the Editor, discussing effect of span on strength of test bar in flexure under impact.

**Engin'g.**—Vol. 73, 1902, p. 189.
Impact Tests on Notched Steel Bars.—Editorial.
Summary of recent experiments.

**Eng. News.**—Vol. 22, 1889, p. 258.
Proper Tests for Cast Iron.—J. B. Johnson.
Recommends slow transverse test with measurements of deflection.

**Eng. News.**—Vol. 40, 1898, p. 128.
Laboratory Apparatus for Impact Tests.
Abstract of paper before Soc. Prom. Eng. Ed., by W. K. Hatt.

**Eng. News.**—Vol. 42, 1899, p. 321.
Testing the Resilience of Steel.—Editorial.

**Eng. Rec.**—Vol. 37, 1897, p. 115.
Impact Testing of Materials.—Editorial.
Criticises the use of notched test bars; recommends impact tests.

**Inst. Civ. Eng.**—Proc., Vol. 112, 1892, p. 442.
Longitudinal and Transverse Impact Tests under Low Temperatures.
Abstract of article by Capts. Kuczera and Reinisch.
See Mittheilungen über Gegenstände des Artillerie und Genie-Wesens, 1892, p. 679.

**Jour. Fr. Inst.**—Vol. 145, 1898, pp. 243–244.
Fatigue of Metal in Wrought Iron and Steel Forgings.—H. F. J. Porter.
Quotes Tetmayer's drop tests.
See also Johnson's Materials of Construction, pp. 398.

**Jour. Fr. Inst.**—Vol. 145, 1898, pp. 414–415.
Commercial Methods of Testing Iron and Steel.—P. Kreuzpointner.
Object of drop test; effect on metal.

**Materials of Engineering.**—R. H. Thurston. Part 3.
Sec. 172—Compression of Rolled Copper by Impact.
Efficiency of Drop Hammers.
Sec. 195—Impact Tests on Manganese Bronze Bars.

**R. R. Gaz.**—Vol. 27, 1895, p. 339.
Heavy and Hard Rails on the Boston and Albany R. R.—P. H. Dudley.
Experiments on rails; relation between deflection from static and dynamic loading.

**R. R. Gaz.**—Vol. 32, 1900, p. 99.
Rail Steel; its Chemistry and Heat Treatment.—W. R. Webster.
Calls attention to the value of the drop test as a check on the finishing temperature of metal.

**The Engr.**—Vol. 86, 1898, p. 1.
Testing Materials for Rolling Stock.
Describes methods and results of impact tests on rolling stock.

## IV. REFERENCE TO GERMAN LITERATURE.

UNCLASSIFIED.

**Bamkd.**—Vol. 1, No. 7, p. 7.
Schlagversuche mit Glasplatten. (Impact Tests on Glass Plates.)

**Bamkd.**—Vol. 1, No. 24–25, p. 361.
Würdigung der Stauchprobe und Aufstellung von Vorschriften für ihre Ausführung. (Value of Upsetting Tests and Specifications for its Execution.)

**Bamkd.**—Vol. 3, No. 10–11, p. 152.
Einheitliche Prüfungsverfahren für Gusseisen. (Standard Method of Testing Cast Iron.)—A. Martens.

**Bamkd.**—Vol. 4, Nos. 16–26, 1899, Vol. 5, Nos. 1–4, 1900.
Weitere Studien über Schienenstahl mit besonderer Berücksichtigung des Basischen Martinstahles. (Further Studies on Rail Steel, especially Basic Open-hearth Steel.)—Anton R. von Dormus.

**Beschlusse der Conferenzen** zu München, Dresden, Berlin und Wien über einheitliche Untersuchungsmethoden bei der Prüfung von Bauund Constructionsmaterialien auf ihre mechanischen Eigenschaften. (Resolutions of the Conferences at Munich, Dresden, Berlin and Vienna, Concerning Standard Methods of Testing the Mechanical Properties of the Materials of Construction.)—J. Bauschinger. Th. Ackermann, 1893. Munich, Germany.

**Chem. Zeit'g.**—Vol. 22, 1898, pp. 1034–5.

Bestimmungen des Widerstandes der Cemente gegen Stoss und Zerspringen. (Determination of the Resistance of Cements against Shock and Bursting.)—Eugen Ackerman.

See also Stone, August, 1899, p. 243; and Cement and Engineering News, September, 1899, p. 44.

**Das Gesetz der Proportionalen Widerstande und seine Anwendungen.** (The Law of Proportional Resistance and Its Application.)—Fr. Kick. Pp. 52–60, 87, 114. A. Felix. Leipzig, 1885.

**Der Civiling.**—Vol. 33, 1887, p. 397.

Einheitliche Untersuchungensmethoden bei der Prüfung von Bau-und Constructionsmaterialien auf ihre mechanischen Eigenschaften. (Standard Methods of Testing the Mechanical Properties of the Materials of Construction.)

**Der Civiling.**—1892, p. 109.

Impact Tests in Flexure at Low Temperatures.—Koepke and Hartig.

See also Stahl und Eisen, 1892, p. 599; and Inst. Civ. Eng.—Proc., Vol. 109, 1892, p. 475.

**Handbuch der Materialienkunde fur den Maschinenbau.** —A. Martens. J. Springer, Berlin, 1898. Pp. 161–196.

Description of impact machines; directions for making impact tests; influence of shape of test piece, etc.

**Handbuch der Ingenieurwissenschaften.**—Vol. 4. **Die Baumaschinen.** —F. Lincke. P. 50. Wilh. Engelmann, Leipzig, 1889.

Chap. XVII: Hilfsmittel und Verfahren der Materialprüfung. (Appliances and Methods for Testing Materials.)—M. Rudeloff.

**Mitth. Berlin.**—Part I, Vol. 6, 1888, p. 36.

Ueber die Aichung von Fallwerken durch Normalkupferkörper. (On the Calibration of Impact Machines by Means of Standard Copper Test Pieces.)—A. Martens.

**Mitth. Berlin.**—Supplement I, 1890, p. 7.

Versuche über die Beiz-und Rostsprödigkeit des Eisens und Stahls. (Tests on the Brittleness of Iron and Steel as Influenced by Acid and Rust.)—A. Ledebur.

**Mittl. Berlin.**—Supplement II, 1890, p. 29.

Untersuchungen mit Eisenbahnmaterialien. (Tests on Railroad Materials.) Impact tests on rails—A. Martens.

**Mitth. Berlin.**—Vol. 9, Part I, 1891, p. 1; Part II, p. 53.

Materialprüfung durch Schlagversuche. (Testing of Materials by Impact.)—A. Martens.

**Mitth. Berlin.**—Vol. 11, Part IV, 1893, p. 177.

Bericht über die im Auftrage des Herrn Ministers für Handel und Gerwerbe ausgeführten vergleichenden Untersuchungen von Seilverbindungen für Fahrstuhlbetrieb. (Report on Comparative Tests on Splices for Elevator Cables, Executed at the Direction of the Secretary of Commerce and Industry.)—M. Rudeloff.

**Mitth. Berlin.**—Vol. 12, Part I, 1894, p. 14.

Untersuchungen von Pappen als Material zur Bekleidung von Baracken oder beweglichen Gebäulichkeiten. (Tests on Cardboard as a Protective Covering for Barracks or Movable Structures.)—M. Rudeloff.

**Mitth. Berlin.**—Vol. 12, Part II, 1894, p. 123.

Ergebnisse von Vorversuchen über die Festigkeitseigenschaften von Kupfer. (Results of Preliminary Tests on the Mechanical Properties of Copper.)—A. Martens

**Mitt. Berlin.**—Vol. 13, Part V, 1895, p. 197.

Untersuchungen über den Einfluss der Kälte auf die Festigkeitseigenschaften von Eisen und Stahl. (Tests on the Influence of Cold on the Mechanical Properties of Iron and Steel.)—M. Rudeloff.

**Mitth. Berlin.**—Supplement I, 1896, p. 9.

Mittheilungen aus dem officiellen Bericht der Französischen Ministerium der offentlichen Arbeiten gebileten "Commission des Methodes d'Essai des materiaux de Construction." (Extracts from the Official Report of the "Commission on the Methods of Testing the Materials of Construction," appointed by the French Ministry of Public Works.) Translated by P. Zizmann.

**Mitth. Berlin.**—Vol. 15, Part II, 1897, p. 119.

Einfluss des Prüfungsverfahrens auf das Ergebniss der Biegeproben bei niederen Warmgraden. (Influence of Methods of Testing on the Results in Bending Tests at Low Temperatures.)—M. Rudeloff.

**Mitth. Berlin.**—Vol. 15, Part VI, 1897, p. 281.

Untersuchungen von Kies und Steinschlag zur Beurtheilung ihres Werthes als Stopfmaterial für den Eisenbahnoberbau. (Tests on Gravel and Broken Stone for Determining their Value as Ballast on Railroad Tracks.)—M. Rudeloff.

**Mitth. Munchen.**—Part 23, p. 59–62.

Bestimmungen über die Art und Weise der Ausführung von Fallversuchen (Schlagproben) besonders auf Grund der von Verband deutscher Eisenbahn-Verwaltungen sowie ausländischer Verwaltungen mitgetheilten Erfahrungen. (Directions for Executing Impact Tests, Based on the Reported Experiences of the German Association of Railroad Management and Foreign Societies.)

**Mitth. Munchen.**—Part 23, p. 68.

Bericht über Stückproben unter dem Fallwerk. (Report on Piece-tests under Impact.)—Rotter and Sailler.

**Mitth. Wien.**—New Series, No. 4, 1894, p. 46.

Wiederstandsfähigkeit einiger natürlicher Gesteine gegen Schlagwirkung. (Resistance of some Natural Rocks to Impact.)—K. Berger.

**Mitth. Wien.**—1893, p. 291; 1897, pp. 7–8.

Ueber den Einfluss der Versuchslänge auf die Festigkeit von Hanfseilen. (On the Influence of Length of Specimen on the Strength of Hemp Ropes.)—B. Kirsch.

See also Mitth. Berlin, Vol. 15, Part V, 1897, p. 250.

**Mitth. Zurich.**—Vol. IV (French translation), p. 99.

Essais au Choc. (Impact Tests.)

**Stahl und Eisen.**—Vol. 17, pp. 723–728.

Influence of Methods of Testing on Results in Bending Tests (including Impact Tests) on Wrought Iron and Steel.—M. Rudeloff.

**Stahl und Eisen.**—1892, pp. 196–200.

Impact Tests on Steel in Flexure at Low Temperatures.—Mehrtens.

**Stahl und Eisen.**—Vol. 22, 1902, pp. 374–380, pp. 425–432.

Prüfung von Eisen und Stahl an Eingekerbten Stücken. (Testing of Iron and Steel by means of Notched Specimens.)—M. Rudeloff.

A discussion of the value of the drop test on notched bars of iron and steel.

**Ueber Eisenbahnschienen.**—A. V. Kerpley, p. 28. A. Felix, Leipzig, 1878.

**Zeitschr. Ver. Deutsch. Ing.**—November 14, 1891; November 28, 1891.

Compression tests of material under falling weights.—A. Martens.

**Zeitschr. Ver. Deutsch. Ing.**—June 8, 1901.

Zugversuche mit Eingekerbten Probekörpern. (Tensile Tests on Notched Specimens.)—A. Martens.

**Zeitschr. Berg. Hutt. u. Sal.**—Vol. 38, 1890, pp. 38–43, 56–57.

Schmelzversuche mit Ferro-Silicium. (Melting Tests with Ferro-Silicon.)—Jüngst.

Results of impact tests on cast iron plates, bedded in sand.

# APPENDIX III.

## RULES FOR STANDARD TESTS OF MATERIALS FORMULATED BY THE GERMAN ASSOCIATION FOR TESTING MATERIALS.*

### 1900.

NOTE.—The German Association for Testing Materials disclaims any intention of enforcing rules, but aims to determine through the knowledge and experience of its members the best possible methods for the testing of materials, in the present state of the science.

### PREFACE.

The German Association for Testing Materials at its first meeting, held in Karlsruhe, on October 25, 1896, assigned to Committee 1 the following task:

*To compare the resolutions of former conferences on standard methods for testing materials with those of foreign associations.*

The Committee decided to form two sub-committees, 1a and 1b, the former to consider the testing of metals, the other, the testing of building materials. These sub-committees were constituted as follows:

SUB-COMMITTEE 1a.

H. Otto, Chief Engineer, Essen (Chairman).
Professor C. von Bach, Building Inspector, Director of the Laboratory for Testing Materials, Stuttgart.
State Counsellor von Borries, Hanover.
G. Eckermann, Chief Engineer, Hamburg.
Adolf Halfmann, Government Architect, St. Johann on the Saar.
Georg Heckmann, Manufacturer, Duisburg-Hochfeld.
Kohn, Railroad Manager, Essen.
Marx, Manager of Bismarck mine near Schwientochlowitz.
Bendix Meyer, Engineer, Gleiwitz (deceased).

* The Executive Committee has directed the translation of this report from the German, and its publication in this volume as a matter of general interest, and especially in its bearing on the work of International Committee No. 22.

Chief Counsellor Rauchfuss, Manager, Germania Docks, Kiel.
Professor M. Rudeloff, Charlottenburg.
E. Schroedter, Business Manager of the Society of German Iron Masters, Düsseldorf.
Comm. Counsellor Gust. Selve, Altena i. W.
Professor Stribeck, Grünewald, near Berlin.

The labors of Sub-Committee 1a are brought to a conclusion with this report, and its decisions are referred to the Association for acceptance at this annual meeting (1900). In arriving at these decisions, the Committee relied as much as possible on the former "*Decisions of the Conferences of Munich, Dresden, Berlin and Vienna, on Standard Methods for Testing Materials, collated by J. Bauschinger*," and considered, as far as practicable, similar labors in other countries, *e. g.*, those of the French *Commission on Methods of Testing Materials*, and also the labors of the *International Association for Testing Materials*.

The section on cast iron is withheld, because an extended series of tests is now in progress in America looking to the standardizing of methods for testing cast iron. The Sub-Committee concluded to await the publication of the results of these investigations, and in the meantime to give German scientists an opportunity to express their views on this question. It therefore induced the Council to send to the Society of Iron Foundries of Germany the resolutions of former meetings on the testing of cast iron with the request for an expression of views. The section on cast iron can therefore not be completed before a future meeting of the Association.

The Sub-Committee has also requested the Council of the Association to direct the attention of public laboratories for testing materials to the desirability of further improvements in the calibration of testing machines and appliances.

Sub-Committee 1a is especially indebted to its Chairman, Mr. Otto, and to its member, Mr. Halfmann, for their untiring zeal in digesting the numerous reports, in tabulating the views of the members, and in editing this material.

SUB-COMMITTEE 1b.

State Counsellor Dr. Hecht (Chairman).
Rud. Dyckerhoff, Manufacturer, Biebrich on the Rhine.
M. Gary, Engineer, Charlottenburg.

Dr. Leube, Manufacturer, Ulm on the Danube.
Dr. F. M. Meyer, Manager, Malstatt, Saarbrücken.
Dr. W. Michaelis, Cement Expert, Berlin.
P. Rasche, Court Master-Mason, Berlin.
F. Schott, Manager, Cement factory, Heidelberg.
Zimmermann, Building Inspector, Hamburg.

The work of Sub-Committee 1b is necessarily dependent on the extensive labors that have been in progress for several years, on the part of the Association of German Portland Cement Manufacturers, with a view of revising the Prussian standards for the delivery and testing of Portland cement. The Sub-Committee is therefore not yet prepared to make a final report upon its work. It is to be hoped that the labors of the above Association, together with those of our Sub-Committee 1b, may lead to the establishment of German rules for the testing of cements and hydraulic mortars.

It is likewise to be hoped that similar efforts in other branches of the building trade, *e. g.*, the lime, plaster and beton industries, may lead to the establishment of standard specifications and methods of testing in Germany. Our Sub-Committee 1b will no doubt keep in touch with these endeavors and strive to further them as much as possible.

A. MARTENS,
*Chairman of Committee* 1.

*Berlin, April*, 1900.

# CONTENTS.

I. GENERAL RULES FOR THE TESTING OF MATERIALS.

1. Description of materials. 2. Calibration of apparatus. 3 and 4. Degree of accuracy. 5. Published reports.

II. MECHANICAL TESTS.

1. TENSION AND COMPRESSION TESTS.

A. General Rules.

6. Character of tests. 7. Influence of time.

B. Static Tests.

a. Machines and Measuring Apparatus.

8. Smooth action. 9. Frequency of calibration.

b. Gripping Devices.

11. Uniform distribution of stress. 12. Compression tests. 13 to 17. Tension tests.

c. Character and Form of Test Specimens.

18 to 20. Compression tests: Bearing surfaces, cubes, relative length. 21 to 30. Tension tests: Influence of size and form of cross-section, gaged length, grip heads, relative dimensions.

d. Method of Procedure.

31. Character of tension test.

α. Abridged Tension Test.

32. Scope of the test.

β. Complete Tension Test.

33 and 34. Scope of the test.

γ. Percentage of Elongation.

35 and 36. By a series of divisions. 37. Between end marks.

C. Impact Tests.

a. General Rule.

b. Impact Testing Machines.

39 and 40. Construction of machines. 41 to 45. Form of hammer. 46 and 47. Guides. 48 and 49. Face of hammer. 50. Supports. 51. Anvil. 52. Foundation. 53. Measurement of drop. 54 and 55. Release of hammer.

c. Method of Procedure.

56 and 57. Method of procedure.

2. OTHER TESTS.

A. Bending Test.

a. General Rules.

58 and 59. Machine of constant movement. 60 and 61. Criterion for judging the material. Standard curvature.

b. Bending Apparatus.

62 and 63. Machine. 64 to 66. Mandrel.

c. Character and Form of Test Pieces.

67 to 71. Condition of material. 72 to 75. Form and preparation of specimens.

d. Method of Procedure.

76 to 79. Cold and hot bending tests at blue heat and red heat. 80 to 82. Determination of radius of curvature.

B. Reverse Bending Test (Wire).

a. Apparatus.

83. Radius of curvature. 84. Avoidance of tension, lever arm.

b. Method of Procedure.

85 and 86. Number of bends.

C. Torsion Test (Wires).

87 and 88. Apparatus, relative length of test piece, avoidance of tension.

D. Forging Tests.

a. Flattening Test.

89. Procedure. 90. Form of specimen. 91. Extent of flattening.

b. Upsetting Test.

92. Procedure. 93. Form of specimen. 94. Extent of the upsetting.

c. Drifting Test.

95 to 97. Procedure. 98. Extent of drifting.

d. Punching Test.

99. Procedure and form of test piece.

III. TESTING OF MATERIALS FOR SPECIAL PURPOSES.

1. WROUGHT IRON AND STEEL.

A. General Rules.

100. Removal of test specimens. 101. Influence of shearing. 102 and 103. Straightening. 104. Heating. 105, 106 and 107. Test pieces of sheared and unsheared plates.

B. Rails.

C. Axles.

D. Tires.

E. Bridge Material.

a. Wrought Iron (Weld Iron).

116 and 117. Methods of testing.

b. Soft and Medium Steel (Ingot Iron).

118 and 119. Methods of testing.

F. Boiler Material.

a. Wrought Iron (Weld Iron).

Methods of testing: 120 to 122. Plates. 123 to 125. Angle Iron. 126. Rivet iron. 127. Welding test.

b. Soft and Medium Steel (Ingot Iron).

128. Methods of testing.

G. Ship Building Material.

a. Wrought Iron (Weld Iron).

129. Methods of testing.

b. Soft, Medium and Hard Steel (Ingot Iron and Ingot Steel).

130 to 132. Methods of testing, length of test pieces.

H. Wire.

133 to 135. Methods of testing, length of test pieces.

1. WIRE CABLES.

136 to 138. Methods of testing, length of test pieces.

2. CAST IRON.

139 and 140. Conclusions deferred.

3. COPPER, BRONZE AND OTHER METALS.

*a.* Copper Plates, Sheets and Rods.

141. Methods of testing.

*β.* Copper Wire.

142. Methods of testing.

*γ.* Preparation and Method of Procedure.

143 to 145. Delivery condition, soft and normal condition. 146 to 151. Removal of test pieces, trueing up, manipulation of specimens. 152 to 156. Method of procedure.

Other Metals and Alloys.

157 and 158. Methods of testing.

Resolutions Adopted at the Fourth Meeting at Rudesheim, September 29, 1900.

# RULES FOR STANDARD TESTS OF MATERIALS.

## I. General Rules for the Testing of Materials.

1. In tests for scientific purposes it is necessary to determine, as far as possible, the character of the material, its natural origin and manufacture, its mechanical, physical, microscopic and chemical properties, and to cite these in published reports together with an account of the treatment of the test-specimen before testing.

2. Every device to be used for the testing of materials must be so constructed, that it may be easily and reliably calibrated.

3. In the case of practical tests, it is not advisable to extend the degree of accuracy of testing devices and results beyond the limits imposed by the inevitable imperfection of the material.

4. In the case of purely scientific tests it is of course essential to aim at the greatest degree of accuracy attainable under given circumstances.

5. A brief description of the apparatus and methods employed in tests should accompany published reports, in order that the value of the results, and especially the degree of their accuracy, may be fairly judged.

## II. Mechanical Tests.

### 1. Tension and Compression Tests.

#### *A. General Rules.*

6. Materials should be tested mainly for stresses of the character to which they will be subjected in practice.

7. The influence of time on the results of strength tests should be recognized in a general way. Since it may be assumed, however, that tests of extraordinary rapidity will not be undertaken, no especial reason exists at present for prescribing the maintenance of a fixed rate for testing in practice the principal materials of construction, *e. g.*, iron in its various forms, copper and bronze.

### *B. Static Tests.*

#### *a.* Machines and Measuring Apparatus.

8. Testing machines should be so constructed that, with careful handling, they shall work smoothly (free from shock).

9. Machines and implements should be calibrated with sufficient frequency.

10. NOTE.—The Association, through its Council, has recommended to the public laboratories for testing materials further improvement in the calibration of testing machines.

#### *b.* Gripping Devices.

11. A gripping device must be so constructed, that the tensile or compressive stress will be distributed as uniformly as possible over the cross-section of the test piece.

Essential requirements are:

12. For compression tests: free and easy motion in every direction of one of the two smooth and plane bearing surfaces (ball-bearing).

13. For tension tests: free and easy motion for the proper axial adjustment of the bar at the beginning of the test.

Experience shows that these conditions are fulfilled:

14. For cylindrical specimens: by spherical bearings, preferably with unsplit journals.

15. For flat specimens: by pin bearings, provided the pin-holes are accurately centred.

16. Enlarged ends with screw-threads and corresponding wedges, provided eccentric loading is precluded.

17. Wedges with rough faces, provided the specimen is centrally gripped.

#### *c.* Character and Form of Test Pieces.

*For Compression Tests:*

18. The bearing surfaces of the test piece should be as nearly plane and parallel as possible and normal to the longitudinal axis; they should, so far as practicable, be prepared by planing, filing, or turning.

19. If possible, the test pieces should be cubic.

20. Where a square cross-section is impracticable, it is advisable to make the length $l$ of the prismatic specimen equal

to $\sqrt{f}$ ($f$ = area of cross-section) since the strength is considerably affected by the ratio $l / \sqrt{f}$.

*For Tension Tests:*

21. Experience shows that, in the case of long specimens, the size and form of the cross-section do not affect the strength.

22. In the case of ductile materials the length-ratio $n = l / \sqrt{f}$ ($l$ gaged length and $f$ = area of cross-section) affects the percentage of elongation considerably; it is therefore necessary to indicate the value of $n$ employed (in the simplest way as index, *e. g.*, $\delta_{5.65} = x\%$ or $\delta_{11.3} = y\%$).

23. Without a knowledge of $n$ the percentage of elongation is an undefined quantity and of no value as a criterion in the case of a ductile material.

24. It is recommended to base the ratio on the round section used in many countries, viz, 20 mm. ($\frac{3}{4}$ in.) diameter, and 200 mm. (8 in.) gaged length, and adopt as *normal ratio* $l = 11.3 \sqrt{f}$.

25. For short specimens the gaged length $l = 5.65 \sqrt{f}$ is recommended.

26. On account of the influence of the enlarged ends (or of the wedge-grips in the case of plain bars) on the elongation, it is advisable to also vary the length $l_g$ of the prismatic portion of the specimen and the length $L$ between the enlarged ends or the wedge-grips according to $\sqrt{f}$.

Th following ratios are proposed:

27. For round sections (Figs. A and B):

$$l = 11.3 \sqrt{f};\ l_g = 12.5 \sqrt{f};\ L = 20 \sqrt{f};\ \text{or}$$

$$l = 5.65 \sqrt{f};\ l_g = 7.0 \sqrt{f};\ L = 12 \sqrt{f}.$$

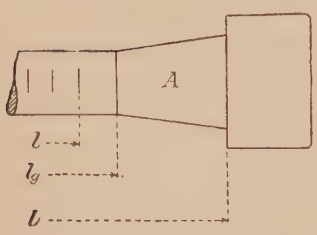



FIG. A.—Enlarged End.

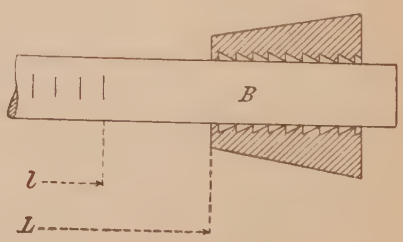



FIG. B.—Plain End.

28. For rectangular sections (Fig. C):

$$l = 11.3\sqrt{f}; \quad l_g = 12.5\sqrt{f}; \quad L = 12.5\sqrt{f} + 2b \text{ or}$$
$$l = 5.65\sqrt{f}; \quad l_g = 7.0\sqrt{f}; \quad L = 7.0\sqrt{f} + 1.5b.$$

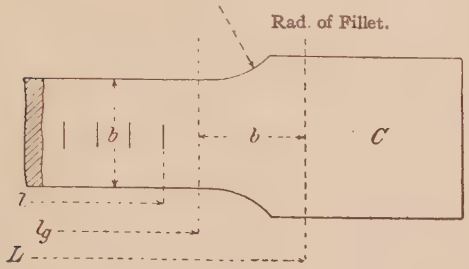
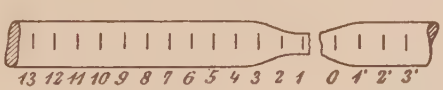



FIG. C.—Enlarged End.

(In this case $b$ = width of prismatic section and $L$ distance between the ends of the wedge-grips.)

29. In the case of round sections the measured length should be ten times the diameter.

30. In the case of rectangular sections the measured length should be 200 mm. (8 in.) for bars whose area of cross-section




exceeds 300 sq. mm., (0.47 sq. in.) and 160 mm. (6.3 in.) for bars whose area of cross-section is from 200 to 300 sq. mm., (0.31 to 0.47 sq. in.).

These standards are proposed with due regard to the fact that the specifications for sheet and plate iron are commonly based on a gaged length of 200 mm. (8 in.) and that the proportions recommended are well adapted to practical requirements in the preparation of the test pieces.

Rectangular bars of varying thicknesses may thus be prepared at a single operation. Round bars have in any case to be prepared separately. The directions in section 27 may thus be easily followed.

### *d.* Method of Procedure.

31. The tension test may be either abridged, or complete.

*α.* Abridged Tension Test.

32. The elements to be determined are:

The ultimate tensile strength.

The elongation of the original gaged length, after fracture.

*β.* Complete Tension Test.

33. In addition to the requirements under section 32, the following data should be determined:

The reduction of cross-section at the point of fracture, the total and the elastic changes of length, and the permanent set under varying loads, whence the following elements may be derived:

the modulus of elasticity,

the proportional limit,

the yield-point,

the elastic limit, *i. e.*, the unit stress within which the permanent set is practically imperceptible.

34. In the case of a complete tension test all data should be fully stated.

*γ.* Percentage of Elongation.

35. The measurement of the elongation is to be based either on a series of divisions marked on the specimen or on the distance between the end marks of the gaged length (the method in practice).

36. The length of the divisions should be 1-20th of the gaged length.

The divisions can be easily laid off by means of a notched scale. In practice the cross-sections (*f*) should be so chosen as to require only a limited number of notched scales.

Since the reductions of area are practically symmetrical on opposite sides of the fracture, the elongation may always be determined as though the fracture had occurred in the middle of the specimen, as will appear from the following example:

The elongation $\lambda$ is found from the measurements $\lambda = (0 \text{ to } 10 + 0 \text{ to } 3' + 3 \text{ to } 10) - l$ and from that the percentage of elongation $\delta_{11.3} = \lambda / l$ per cent.

37. In determining the percentage of elongation by measuring the distance between the end marks (the method in practice)

in the case of ductile materials, tests in which the fracture occurs within the end quarter-lengths should be rejected.

### *C. Impact Tests.*

#### *a.* General Rule.

38. The shock-resisting capacity of a material can be determined only by impact tests.

#### *b.* Impact Testing Machines.

Impact-testing machines should meet the following requirements:

39. Impact-testing machines of not more than 6m. (19.7 ft.) drop can be more easily housed and more reliably constructed than higher ones; it is therefore recommended not to exceed this height in future construction and to build the superstructure of steel.

40. Provision should be made for preventing the test piece from bounding from its supports and from tipping, without interfering, however, with its free movement.

41. The normal weight of the hammer should be 1000 or 500 kg. (2200 or 1100 lbs.); in special cases smaller weights are admissible.

42. The hammer may be made of cast iron, cast or forged steel.

43. The hammer should be so shaped as to bring the center of gravity as low as possible.

44. The axis of gravity of the hammer must coincide with the center lines of the guides.

45. The ratio of the guide-length of the hammer to the clear width between the guides should be greater than 2 to 1.

46. The guides should be made of metal, *e. g.*, steel rails, and so constructed that the hammer shall have little play.

47. Graphite is recommended for the lubrication of the guides.

48. The face of the hammer should be of steel, and secured by dove-tailed wedging concentric with the axis of gravity of the hammer.

49. With hammers of 1000 and 500 kg. (2200 and 1100 lbs.) the face of the hammer should be rounded off to a radius of 150 mm. (5.9 in.) and the line of contact should be normal to the axis of gravity.

50. The supports of the test piece should be securely fastened to the anvil, *e. g.*, by wedging.

51. The anvil should consist of a single block of cast iron and its weight should be at least ten times that of the hammer.

52. The foundation should be of masonry, its dimensions depending on the nature of the ground, but the height should be not less than 1 m. (3.28 ft.).

53. The scale for reading the height of drop should be attached adjustably to the guide backing and be divided into cm. (0.394 in.).

54. The device for tripping the hammer should be so constructed as not to interfere with its free fall.

55. Some safety device should be provided to prevent the accidental release of the hammer.

*c.* Method of Procedure.

56. Before beginning the test it should be seen to, that the guides are perpendicular and that the hammer moves freely throughout the range of travel.

57. To facilitate the comparison of results, it is recommended that the details of the method be fully stated.

2. OTHER TESTS.

*A. Bending Test.*

*a.* General Rules.

58. The bending test should be made with a machine of slow and constant movement.

59. The point of maximum tensile fiber stress should be fully exposed to view.

60. The ratio between the thickness of the specimen $a$ and the radius of curvature $\rho$ of the neutral plane on the appearance of plainly visible cracks is the best criterion for judging the material.

61. The standard for curvature shall be: $C = 50\, a / \rho$.

*b.* Bending Apparatus.

The machine should operate:

62. either by pressure at the center of a bar supported at both ends, or

63. by pressure at the end of a bar fastened at one end.

The bending may be made:

64. either freely about the rounded edge of the bearing block, or the end grip;

65. or by forcing the inner surface of the specimen to a snug fit about a mandrel.

66. In bending about a mandrel its diameter should depend on the character of the material and the thickness of the specimen.

*c.* Character and Form of the Test Pieces.

The material may be tested:

67. In its *normal condition;* in which case the test piece should be removed cold, and, if possible, without suffering any change of form (not with shears, see sections 100 and 101);

68. *heated;* in which case the heating should be done in a manner adapted to the material;

69. *quenched, i. e.*, heated and then quickly cooled by being stirred in water from 15 to 30° C.;

70. *undeformed:* prismatic bars (see sections 72 and 73);

71. *deformed:* notched bars, punched or drilled bars, etc. (see sections 74 and 75).

72. The test specimens should as a rule be rectangular in section and the edges should be rounded off considerably at the section of maximum bending.

73. In the case of rolled shapes the sectional area should be preserved as far as practicable.

74. The notching of the tensile side should be done by planing with a sharp tool up to 0.2 of the thickness of the specimen.

75. The punching should be done if possible, with a punch of a diameter $2a$ at the centre of a bar with the relative dimensions, $a$ (width): $b$ (depth) $= 1 : 5$.

*d.* Method of Procedure.

The specimen may be subjected to:

76. *The cold bending test*

($a$) at normal temperature (10 to 30° C.),

($b$) artificially cooled (below 0° C.).

77. *The hot bending test*

($a$) at blue heat,

($b$) at red heat.

78. When tested at a blue heat the metal must be heated till it turns blue on a brightened surface and retains this color (it will not turn blue if it is too cold, and will not stay blue if it is too hot).

79. When tested at a red heat the metal should be at a temperature of 500 to 600° C. (it will then appear plainly red in the shade).

80. The radius of curvature may be found with sufficient accuracy by means of curved gages applied to the tensile side of the bar.

81. The radii of the gages should increase progressively by 2 mm. (0.079 in.).

82. The arc of the gages should always be 45°.

### *B. Reverse Bending Test (Wire).*

#### *a.* Apparatus.

83. Reverse bending tests should be made about rounded edges whose radius of curvature is equal to twice the thickness of the specimen ($r = 2\ a$).

84. The free end of the specimen must be so gripped that no tensile stress shall be developed and the lever arm $h$, measured from the beginning of curvature to the grip shall be equal to fifteen times the thickness of the specimen ($h = 15\ a$).

#### *b.* Method of Procedure.

85. The complete bend consists in bending the wire from its normal position 90° to the right and back; the second complete bend is then made 90° to the left and back; the third to the right, etc.

86. The value of the material depends on the number of bends sustained before fracture.

### *C. Torsion Test (wire.)*

87. The torsion test should be made with a machine which registers the number of twists up to fracture, and the free length of the specimen shall be equal to seventy-five times the thickness ($l = 75\ a$).

88. The apparatus should not oppose the free change of length of the specimen.

### *D. Forging Test.*

#### *a.* Flattening Tests.

89. The flattening test is to be made with a hand hammer, sledge hammer or rapid power hammer. The hammer face should be rounded to a diameter of 15 mm. (0.6 in.) and applied transversely to the desired direction of the flattening.

90. The test specimen shall have the relative dimensions $a$ (thickness): $b$ (width) $= 1 : 3$ and shall be widened or stretched on a length $l = 1.5$ to 2 b.

91. The standard for the widening ($w$) from $b$ to $b'$ and of the stretching ($s$) from $l$ to $l'$ is the ratio $b' / b$ and $l' / l$ respectively, reached at the beginning of tearing.

#### *b.* Upsetting Test.

92. The upsetting test is to be made with a hammer or a rapid power hammer.

93. The test pieces should be cylinders of a height equal to twice their diameter ($h = 2\ d$).

94. The standard for the upsetting ($u$) is the ratio $h—h' / h$ reached at the beginning of cracking.

#### *c.* Drifting Test.

95. The drifting test consists in punching at a bright red heat a bar of the relative dimensions, $a : b = 1 : 5$, to a diameter $2\ a$ and then enlarging the hole with a drift pin.

96. The diameter of the drift pin should have a taper of 1 to 10.

97. If necessary the test piece should be reheated.

98. The standard for the enlargement ($E$) is the ratio $d' / d$ reached at the beginning of tearing.

#### *d.* Punching Test.

99. The punching test consists in placing on the anvil a red hot specimen, whose width-ratio $a : b$ is greater than $1 : 5$, and determining with a punch of diameter $a$ how near the edge a hole may be made without cracking.

## III. TESTING OF MATERIALS FOR SPECIAL PURPOSES.

### 1. WROUGHT IRON AND STEEL.

### *A. General Rules.*

100. The test pieces must be removed cold, and as far as

practicable with sharp tools (planes, saws, etc.) without producing bending

101. In case the test pieces for tensile tests are sheared, punched or chiseled off, at least 5 mm. (0.20 in.) must be removed within the gaged length (see section 26) (by planing or reaming), in order to remove the effect of the shearing, etc.

102. Bent pieces should, as a rule, be straightened cold under a press or with copper hammers.

103. If the pieces must be straightened hot, they should not be heated above a cherry-red heat (650° C.).

104. Special care is necessary when the test pieces are to be glowed out. The glowing must take place at 800 to 900° C. and the cooling must be done slowly.

The glow heat must lie above the critical point—about 750° C.—and should not exceed 900° C., to avoid the danger of burning.

105. The rolled surface should be preserved as far as practicable.

106. In the case of sheared plates the test pieces for longitudinal and transverse specimens should be removed from the corresponding longitudinal and transverse sides; in the case of unsheared plates (direct from the rolls) the test pieces should be taken from the edge or end trimmings.

107. The outside strips of unsheared plates to a distance of at least 30 mm. (1.2 in.) must not be used as test pieces.

### *B. Rails.*

108. Rails should be subjected to the simple tension test (section 32) and to the drop test in bending.

109. In the case of the drop test (sections 38 to 57) the projecting ends affect the result; therefore like lengths must be used for like sections and like distances between supports, if the results are to be directly comparable. The edges of the supporting blocks should be rounded off to a radius of 50 mm. (1.97 in.).

### *C. Axles.*

110. Axles should be subjected to the simple tension test (section 32) and to the drop test in bending.

111. In the case of the drop test, the span should be 1.5 m. (4.42 ft.).

*D. Tires.*

112. The material for tires should be subjected to the simple tension test (section 32) and to the drop test in bending (sections 38 and 57).

113. In the case of the drop test the hammer face should strike upon a light intermediate plate, conforming to the outline of the tire, having a plane upper surface, whose weight should be as light as possible and under no circumstances exceed 50 kg. (110.2 lbs.).

114. This intermediate piece as well as the supporting block conforming to the section of the tire, should be shaped to a radius of 150 mm. (5.91 in.).

115. The tire should be held in a perpendicular position by a device which will not interfere with its free bending.

*E. Bridge Material.*

*a.* Wrought Iron (Weld Iron).

116. Wrought iron for bridge building purposes should be subjected to the simple tension test (section 32) and to the bending test.

117. The bending test should be applied to the undeformed specimen (section 70) cold (section 76 *a*) and at red heat (section 79).

*b.* Soft and Medium Steel (Ingot Iron).

118. Soft and medium steel for bridge building purposes should be subjected to the simple tension test (section 32) and to the bending test.

119. The bending test (76 *a*) should be applied to the undeformed specimen (section 70) cold and quenched (sections 69 and 104).

*F. Boiler Materials.*

*a.* Wrought Iron (Weld Iron).

120. Plates should be subjected to the simple tension test (section 32), the bending test without deformation (section 70), cold (section 76 *a*) and at red heat (section 79), as well as to the

flattening test (section 89 to 91) and to the drifting test (section 95 to 98).

121. In testing boiler plates, which have already been subjected to shop manipulations, for the properties which they had before use, the test piece must be taken from those parts which have not been reduced to unequal thicknesses through working and have remained fairly straight.

122. In case only a curved plate is available, then the specimen should be obtained by drilling, planing or by means of a circular saw. The test pieces should be very carefully prepared in other respects according to sections 100 to 104.

123. Angle iron should be subjected to the simple tension test (section 32), to the bending test (sections 83 to 86), to the flattening test (sections 89 to 91) and to the drifting test (sections 95 to 98).

124. Longitudinal strips from the sides should be bent without previous deformation (section 70) cold (section 76 *a*) or at red heat (section 79).

125. The sides, to a length of 100 mm. (3.94 in.) may also be bent apart or folded together until the appearance of cracks.

126. Rivet iron should be subjected to the simple tension test (section 32), to the cold bending test without previous deformation (section 70) and to the cold upsetting test (sections 92 to 94).

127. If necessary the material should be tested for weldability.

*b.* Soft and Medium Steel (Ingot Iron).

128. The same tests should be applied to soft and medium steel as to wrought iron (section 120), the bending tests, however, may be confined to quench tests (section 59) on undeformed specimens (section 70).

## *G. Ship-Building Material.*

*a.* Wrought Iron (Weld Iron).

129. Wrought iron for ship-building purposes should be subjected to the simple tension test (section 32), to the bending test without previous deformation (section 70), cold (section 76 *a*) and at red heat (section 79) as well as to the forging test.

*b.* Soft, Medium and Hard Steel (Ingot Iron and Ingot Steel).

130. Soft, medium and hard steel for ship-building purposes, such as sheets, plates, shapes and rivet metal, should be subjected to the simple tension test (section 32), to the bending test and to the forging tests.

NOTE.—Wrought iron rivets, see section 126.

131. The bending test should be applied to undeformed bars (section 70) quenched (section 69) and at red heat (section 79).

132. If necessary the material should be tested for weldability.

*H. Wire.*

133. Wire should be subjected to the simple tension test (section 32), to the reverse bending test (sections 83 to 86) and to the torsion test (sections 87 and 88).

134. The free length of the specimen in the case of the tension test and especially in that of the torsion test should be seventy-five times its thickness.

135. The gaged length ($l$) in the case of the tension test, is to be fifty times the diameter ($a$).

*I. Wire Cables.*

136. Cables shall be subjected principally to the simple tension test (section 32).

137. The ratio of the free length $L$, of the specimen between grips, to the thickness shall be at least 30.

138. The gage length shall be 25 diameters.

2. CAST IRON.

139. The test pieces for the testing of cast iron shall be 3 cm. (1.08 in.) square and 110 cm. (43.3 in.) long.

140. NOTE.—Conclusions in reference to character and scope of tests, preparation of test specimens, etc., will be deferred pending the hearing of interested parties and the results of investigations in other countries.

3. COPPER, BRONZE AND OTHER METALS.

*a. Copper.*

*a.* Plates, Sheets and Bars.

141. The material should be subjected to the simple tension test (section 32), to the cold and hot bending test (sections 70, 76 *a*, and 79), and in the case of bars, also to the upsetting test (sections 92 to 94).

### β Wire.

142. Copper wire should be subjected to the simple tension test (section 32), to the reverse bending test (sections 83 to 86) and to the torsion test (sections 87 and 88).

### γ. Preparation and Method of Procedure.

#### *Preparation of the Material.*

143. The tests are to be applied to the material as delivered or if desired, in the case of copper plates, sheets or bars also in its soft state.

144. In order to test the properties of copper in its normal condition, the test pieces must be softened.

145. This should be done after removal of the specimens, *i. e.*, before the final finishing, by heating them at a temperature of 600 to 700° C. (but not beyond) in a furnace, then cooling them in the air until the disappearance of the glow and then in water of 15° C.

#### *Removal and Preparation of Test Pieces.*

146. The test pieces should only be removed cold with saw, file or by machine, and subsequent trueing should be avoided if possible.

147. The trueing should invariably be done carefully and if practicable with wooden or copper hammers.

148. For testing copper in its soft condition specimens which in their natural state approach the finished form, may be heated for purposes of trueing, but should in that case be reheated to change them to a soft condition.

149. Since the results in the case of copper depend so much on the manipulation of the specimens, the final finishing should be done with the greatest care, and it is especially important that the travel of the finishing tool be not arrested within the gage length and that the final cuts be very light. The bars should be planed lengthwise and finished with emery paper.

150. In the case of flat bars for tensile tests, the edges should be slightly chamfered by filing.

151. The specimens for the bending test should be prepared according to sections 72 and 73.

*Method of Procedure.*

152. The cold bending test in the case of copper should not be performed at a temperature below 10° C.

153. In the case of test pieces taken from plates, sheets and bars, those specimens which have stood the bending until the ends meet should be pressed together, until the inner surfaces are brought to perfect contact.

154. In the case of the hot bending test the test pieces should be raised to a cherry-red heat (600° C.); and the bending should be continued until cracks appear or until the inner surfaces are brought to perfect contact.

155. The reverse bending test for wire is to be applied cold according to sections 83 to 86.

156. The torsion test for wire is to be made according to sections 87 and 88.

*b.* Other Metals and Alloys.

157. Other metals and alloys should be subjected to the simple tension test (section 32), the compression test, the bending test, and the cold (section 76 *a*) and hot bending test (section 79).

158. The rules for testing such metals and alloys should correspond, according to the properties of the material, to the rules for testing cast iron or those for testing copper; *i. e.*, in the former case, simple tension, compression and bending tests are recommended; in the latter, simple tension and compression tests as well as cold and hot bending tests.

# CHARTER

OF THE

# AMERICAN SOCIETY FOR TESTING MATERIALS.

*To the Honorable, the Judges of the Court of Common Pleas No. 2 in and for the City and County of Philadelphia: of March term, 1902, No. 2056:*

In compliance with the requirements of an Act of the General Assembly of the Commonwealth of Pennsylvania, entitled "An Act to Provide for the Incorporation and Regulation of Certain Corporations," approved the 29th day of April, A. D. one thousand eight hundred and seventy-four, and the supplements thereto, the undersigned, Henry M. Howe, Charles B. Dudley, Edgar Marburg, Robert W. Lesley, Mansfield Merriman, Albert Ladd Colby and William R. Webster, six of whom are citizens of Pennsylvania, having associated themselves together for the purposes hereinafter set forth, and desiring that they may be incorporated according to law, do hereby certify:

1. The name of the proposed corporation is the "American Society for Testing Materials."

2. The corporation is formed for the Promotion of Knowledge of the Materials of Engineering, and the Standardization of Specifications and the Methods of Testing.

3. The business of the said corporation is to be transacted in Philadelphia.

4. The said corporation is to exist perpetually.

5. The names and residences of the incorporators are as follows:

HENRY M. HOWE, 27 West Seventy-third Street, New York.
CHARLES B. DUDLEY, Altoona, Pa.
EDGAR MARBURG, 517 South Forty-first Street, Philadelphia.
ROBERT W. LESLEY, 22 South Fifteenth Street, Philadelphia.
MANSFIELD MERRIMAN, South Bethlehem, Pa.
ALBERT LADD COLBY, South Bethlehem, Pa.
WILLIAM R. WEBSTER, "The Bartram," Thirty-third and Chestnut Streets, Philadelphia.

6. The management of the said corporation shall be vested in an Executive Committee, consisting of six (6) members, viz: the Chairman, the Vice-Chairman, the Secretary, the Treasurer, and two other members of the corporation, and such other officers as the corporation may from time to time appoint.

7. The corporation has no capital stock, and the members thereof shall be composed of the subscribers and their associates and of such persons as may from time to time be admitted by vote in such manner and upon such requirements as may be prescribed by the By-Laws. The corporation shall nevertheless have power to exclude, expel or suspend members for just or legal cause, and in such legal manner as may be ordained and directed by the By-Laws.

8. The By-Laws of this corporation shall be admitted and taken to be its laws subordinate to the statute aforesaid; this Charter; Constitution and Laws of the Commonwealth of Pennsylvania, and the Constitution of the United States; they shall be altered and amended as provided for by the By-Laws themselves; and shall prescribe the powers and functions of the Executive Committee herein mentioned and those to be hereafter elected. The times and places of meetings of the Committee and this corporation; the number of members who shall constitute a quorum at the meetings of the corporation, and of the Committee; the qualifications and manner of electing members; the manner of electing officers; and the power and duties of such officers; and all other concerned and internal arrangements of the said corporation.

Witness our hands and seals this twenty-first day of March, A. D. 1902.

[Signed]

EDGAR MARBURG,
R. W. LESLEY,
WM. R. WEBSTER,
MANSFIELD MERRIMAN,
ALBERT LADD COLBY.

## BY-LAWS.

Adopted June 12, 1902.

### ARTICLE I.

#### Members.

Section 1. Any person, corporation or technical society holding membership in the International Association for Testing Materials is eligible for membership.

Section 2. Any person, corporation or society can become a member of this Society and of the International Association for Testing Materials simultaneously upon being proposed by two members of this Society and being approved by its Executive Committee.

Section 3. Applications for membership and resignation from membership must be transmitted in writing to the Secretary.

### ARTICLE II.

#### Officers and their Election.

Section 1. The officers shall be a President, Vice-President, Secretary and Treasurer.

Section 2. The offices of Secretary and Treasurer shall be held by the same person.

Section 3. These officers shall be elected by letter-ballot, at the Annual Meeting, and shall hold office for two years.

Section 4. The Executive Committee shall consist of these officers and also the last past-President and three members, two being elected by letter-ballot at each Annual Meeting in the odd years and one at each Annual Meeting in the even years.

Section 5. The President shall be, *ex-officio*, the nominee for American Member of the Council of the International Association.

Section 6. The Secretary shall receive a salary to be fixed by the Executive Committee.

Section 7. The officers and members of the Executive Committee of this Society to hold office until the next election under

these By-Laws, shall be as follows: To hold office for two years—President, Charles B. Dudley; Vice-President, R. W. Lesley; Secretary-Treasurer, Edgar Marburg; members of the Executive Committee, Henry M. Howe and James Christie. To hold office for one year—members of the Executive Committee, Albert Ladd Colby and John McLeod.

Section 8. The above officers and members of the Executive Committee, as well as all succeeding officers and members of the Executive Committee elected under these By-Laws, shall serve for the respective terms to which they shall have been elected, or until their successors shall have been duly elected.

Section 9. The Executive Committee shall have the power to fill any vacancies occurring in their number, by death, resignation, or otherwise.

Section 10. The election of officers and members of the Executive Committee shall be by letter-ballot. The Executive Committee, before each Annual Meeting, shall appoint a Nominating Committee, whose duty it shall be to nominate a full list of officers. The list of nominations so made shall be submitted to the membership not more than eight (8) nor less than four (4) weeks before the coming Annual Meeting.

Further nominations, signed by at least ten (10) members, may be submitted to the Secretary in writing at least four (4) weeks before the Annual Meeting, and such nominations shall also be submitted to the membership on the official ballot.

## ARTICLE III.

### Meetings.

Section 1. The Society shall meet annually. The time and place of each meeting shall be fixed by the Executive Committee.

Section 2. Special meetings may be called whenever the Executive Committee shall deem it necessary, or upon the request in writing to the President of twenty-five (25) members.

## ARTICLE IV.

### Dues.

Section 1. The fiscal year shall commence on the first of January, and all dues shall be payable in advance.

Section 2. The annual dues of each member shall be $3.00. Of this amount $1.50 shall be transmitted by the Secretary to the International Association for Testing Materials. The remainder shall be applied to the treasury of the Society.

Section 3. Any member of the Society whose dues shall remain unpaid for the period of one year shall forfeit the privileges of membership. If he neglects to pay his dues within thirty days thereafter, and after notification from the Secretary, his name may be stricken from the roll of membership by the Executive Committee.

## ARTICLE V

### Amendments.

Section 1. Proposed Amendments to these By-Laws, signed by at least three members, must be presented in writing to the Executive Committee at least four weeks before the next Annual Meeting. In the notices for this meeting the proposed Amendment shall be printed. At the Annual Meeting the proposed Amendment may be discussed and amended and may be passed to letter-ballot by a two-thirds vote of those present.

If two-thirds of the votes obtained by letter-ballot are in favor of the proposed Amendment, it shall be adopted.

Section 2. The Executive Committee is authorized to number the Articles and Sections of the By-Laws to correspond with any changes that may be made.

## RULES GOVERNING THE EXECUTIVE COMMITTEE.

1. Regular meetings shall be held on the first Saturday in January, April, July and October. Four members shall constitute a quorum.

At each meeting the Secretary shall report the names of all new members and of members who have resigned during the previous quarter; and shall present a financial statement.

At the January and October meetings, the Secretary shall report the names of all members whose dues are unpaid.

The accounts of the Secretary shall be duly audited at the middle and close of each fiscal year, and the report of the auditors shall be presented in writing at the July and January meetings.

2. Special meetings may be held at any time at the call of the President, or upon the written request of four members of the Executive Committee. The notice for such meetings shall be mailed by the Secretary at least one week in advance of the meeting, and the business shall be stated in the notice.

3. The Secretary shall transmit to the International Association within five days after the first day of January, April, July and October, $1.50 for each member whose dues were paid in the previous quarter together with the names of those members.

No other expenses shall be paid except on vouchers certified to be correct by the Chairman of the Committee on Finance, or a member thereof designated by the Chairman.

## GENERAL INFORMATION.

### INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

*Historical.*—The International Association for Testing Materials had its origin in a conference of a small group of workers in experimental engineering held in Munich in 1882, at the instance chiefly of the late John Bauschinger. Meetings on a larger scale were subsequently held at Dresden (1884), Berlin (1886), Munich (1888), Vienna (1893) and Zurich (1895). At the Zurich Congress the International Association for Testing Materials was formally organized, the Second Congress was held at Stockholm in 1897, the Third Congress met at Buda-Pesth in 1901*, the Fourth Congress will assemble at St. Petersburg in 1904, and succeeding congresses will be convened biennially thereafter.

*Membership.*—The membership, according to the latest official report (December, 1902), is distributed as follows: Russia, 389; Germany, 370; United States of America, 232†; Austria, 193; France, 153; Switzerland, 81; Hungary, 74; Sweden, 55; England, 52; Belgium, 47; Denmark, 44; Norway, 43; Holland, 42; Italy, 42; Spain, 27; Roumania, 20; Portugal, 12; Luxembourg, 5; Servia, 5; Australia, 2; Brazil, 2; Argentine Republic, 1; Chili, 1. Total, 1,892, representing 23 countries.

*Objects.*—The objects of the Association, as set forth in its By-Laws‡ are : "The development and unification of standard methods of testing ; the investigation of the technically important properties of the materials of construction and other materials of technical importance, and also the perfecting of apparatus used for that purpose."

The important subject of specifications has, however, also been included within the scope of the Association's activity. Thus, International Committee No. 1 has been charged to report on the following problem: "On the basis of existing specifications, to seek methods and means for the introduction of inter-

* The Third Congress, originally scheduled for 1900, to be held at Paris during the Exhibition, was abandoned in order not to conflict with the International Testing Congress, conducted under French auspices.

† The American membership is now (January, 1903), 290.

‡ These By-Laws are given in full on pp. 370-3.

national specifications for testing and inspecting iron and steel of all kinds."

Again, in pursuance of American initiative at the Buda-Pesth Congress (1901), Committee No. 1 has been enlarged by the addition of three American members, with a view of reporting on "Standard International Specifications for Cast Iron and Finished Castings," and Committee No. 22 has been instructed to report "On the Feasibility of the Establishment of Standard International Specifications for Cements."

*Administration.*—The affairs of the Association are administered by a Council, consisting of the President and one representative (member of Council) from each country having a membership of twenty (20) or more.

*Methods.*—The original plan was to conduct investigations almost exclusively through the agency of international committees. Twenty-four such committees were appointed and several others are now in process of organization. These committees proved unwieldy, however, by reason of their large membership, with the added difficulties arising from geographical separation and differences of language. In pursuance of resolutions at the Buda-Pesth Congress (1901) the Council has recently discharged some of these committees, reassigning the problems in part to individual referees*. In the case of questions of direct international concern, the original international committees are continued.

At the international congresses the reports of these committees as well as individual contributions by members are presented and discussed.

*Publications.*—On May 5, 1896, the International Council effected an arrangement with the publishers of "Baumaterialenkunde" † (Materials of Construction) by which that journal became the official organ of the Association. Since July, 1896, this journal has published the Proceedings of Congresses and other official matter in German and French. The fact that the Association did not furnish printed Proceedings to members free of charge, and that no provision had been made for translation

* For complete list of problems, committees and referees, see pp. 374-80.

† *Baumaterialenkunde:* Published bi-weekly at Stuttgart, Germany, in German and French. Regular subscription price $3.50 per annum, special terms to members of the International Association for Testing Materials, $2.50 per annum. Address: Staehle and Friedel, No. 57 Tuebinger Street, Stuttgart, Germany.

into English, gave rise to no little dissatisfaction. At the Buda-Pesth Congress (1901) the International Council was accordingly authorized to perfect a new arrangement by which all official matter shall hereafter be published as a Supplement to "Baumaterialenkunde," under the title "Official Communications of the International Association for Testing Materials." This Supplement is to be issued in three separate editions (German, English and French) and is to be sent free of charge to every member of the Association in such language as may be preferred. Final negotiations to that end are now under way between the Council and the publishers.

## ORGANIZATION OF THE AMERICAN MEMBERS OF THE INTERNATIONAL ASSOCIATION.

*Historical.*—With a view of bringing the members of like nationality into closer relations among themselves, and in order to simplify the management and render the work of the Association more effective, it was decided at the Stockholm Congress (1897) to encourage the consolidation of the membership in the various countries into separate national organizations. In pursuance of this action the American members met in Philadelphia on June 16, 1898 and organized under the name of the "American Section of the International Association for Testing Materials."

In March, 1902, the Executive Committee of the American Section applied for a Charter under the laws of the State of Pennsylvania for purposes of incorporation under the proposed new name of the "American Society for Testing Materials." This Charter was duly granted, and at the Fifth Annual Meeting, held in Atlantic City, N. J., it was unanimously adopted on June 12, 1902.

*Objects.*—The objects of the Society are essentially identical with those of the International Association, with which it stands in direct organic relation, both through its membership in the same as a body, and through the prescribed individual membership on the part of every one of its members.

As stated in the Charter: "The corporation is formed for the promotion of knowledge of the materials of engineering, and the standardization of specifications and the methods of testing."

*Representation on the International Council.*—The American members are entitled to one representative on the International

Council. By the new Statutes of the Association (1901): "the members of Council shall be proposed by the members of each country; their final appointment being confirmed by the Congress." According to the By-Laws of the American Society the President becomes, "*ex officio*, the nominee for American Member of the Council of the International Association."

*Meetings.*—The Society meets annually at a time and place fixed by the Executive Committee. Special meetings may also be called in accordance with the provisions of the By-Laws.

Annual meetings have been held in past years as follows:

First Annual Meeting, Philadelphia, Pa., House of Engineers' Club of Philadelphia, August 27, 1898.

Second Annual Meeting, Pittsburg, Pa., Rooms of Engineers' Society of Western Pennsylvania, August 15, 16, 1899.

Third Annual Meeting, New York, N. Y., House of American Society of Mechanical Engineers, October 25, 26, 27, 1900.

Fourth Annual Meeting, Niagara Falls, N. Y., International Hotel, June 29, 1901.

Fifth Annual Meeting, Atlantic City, N. J., Hotel Traymore, June 12, 13, 14, 1902.

*Membership.*—The number of American members at the time of the organization meeting in 1898 was 70. The membership reported at the successive annual meetings was as follows: (1899) 128, (1900) 160, (1901) 168, (1902) 175, and it is now (January, 1903) 290.

*Methods.*—The operations of the Society are conducted in part under the auspices of the International Association and in part independently.

The number of American representatives on international committees is fixed by the International Council. These American sub-committees are authorized, however, to increase their number at pleasure, subject always to the approval of the Executive Committee of the American Society. The sense of these enlarged sub-committees on all questions is determined by majority vote; but on the international committees, the representation and the number of votes allowed remain as originally fixed by the International Council.

The American Society appoints other committees at its discretion entirely independently of the International Association.

Thus, in pursuance of resolutions at the Fifth Annual Meeting, committees have been appointed on "Standard Specifications for Cement," on "Standard Specifications for Paving and Building Brick," and on "Preservative Coatings for Iron and Steel." Other committees, on "Standard Specifications for Cast Iron and Finished Castings," and on "Bitumen," are in course of organization. Where commercial interests are involved, the policy is to accord equal numerical representation on such committees to engineers (or scientists) and to manufacturers.

The reports of the international sub-committees and of the American committees are presented at the annual meetings for discussion in open session and become subject to such action as may be there decided on.

*Publications.*—The publications of the Society have appeared heretofore at irregular intervals in the form of bulletins. Twenty-eight (28) bulletins, containing a total of 266 pages, have been issued.* It has been decided to publish the Proceedings henceforth in the form of annual volumes. The first of these volumes is designated Volume II of the Proceedings, the Bulletins previously issued constituting Volume I.

The most notable work thus far accomplished by the Society is the framing, by the American Branch of International Committee No. 1 (enlarged for this purpose to thirty-four members) and the adoption in August, 1901, by letter-ballot of the Society, of Standard Specifications on (1) Structural Steel for Bridges and Ships, (2) Structural Steel for Buildings, (3) Open-Hearth Boiler Plate and Rivet Steel, (4) Steel Rails, (5) Steel Splice Bars, (6) Steel Axles, (7) Steel Tires, (8) Steel Forgings, (9) Steel Castings, (10) Wrought Iron. These ten Standard Specifications are published separately in the order given as Bulletins Nos. 8 to 16 inclusive, and Bulletin No. 24.

These specifications have attracted wide attention and have served as a basis for formal discussions on the part of leading engineering societies, such as the American Society of Civil Engineers, the American Society of Mechanical Engineers, the American Institute of Mining Engineers, the American Foundrymen's Association, etc.

* Bulletins Nos. 1, 2 and 3 are out of print. Bulletins Nos. 4 to 28, inclusive, may be had on application to the Secretary at a total cost, postage prepaid, of two dollars and fifty cents ($2.50). For Table of Contents of these publications, see pp. 366–8.

# OFFICERS

OF THE

# AMERICAN SOCIETY FOR TESTING MATERIALS.

[Affiliated with the International Association for Testing Materials.]

PRESIDENT:

CHARLES B. DUDLEY.

VICE-PRESIDENT:

R. W. LESLEY.

SECRETARY-TREASURER:

EDGAR MARBURG.

Office: University of Pennsylvania, Philadelphia, Pa.

---

## MEMBERS OF THE EXECUTIVE COMMITTEE:

*Term Expiring in* 1903:

ALBERT LADD COLBY,

JOHN MCLEOD.

*Term Expiring in* 1904:

JAMES CHRISTIE,

HENRY M. HOWE.

---

## STANDING COMMITTEES:

ON FINANCE:

JOHN MCLEOD, *Chairman*,

ALBERT LADD COLBY,

R. W. LESLEY.

ON MEMBERSHIP:

JAMES CHRISTIE, *Chairman*,

R. W. LESLEY,

EDGAR MARBURG.

ON PUBLICATIONS:

HENRY M. HOWE, *Chairman*,

ALBERT LADD COLBY,

EDGAR MARBURG.

## LIST OF MEMBERS

OF THE

## AMERICAN SOCIETY FOR TESTING MATERIALS.

[Affiliated with the International Association for Testing Materials.]

| | Date of membership. |
|---|---|
| ACKERMAN, ERNEST R.<br>President Lawrence Cement Company, 1 Broadway, New York, N. Y. | 1902 |
| AERTSEN, GUILLIAEM<br>General Manager Latrobe Steel Company, 1200 Girard Building, Philadelphia, Pa. | 1898 |
| AIKEN, W. A.<br>General Inspector of Material, Rapid Transit Railroad Commission of New York, 613 Empire Building, Pittsburg, Pa. | 1902 |
| AJAX METAL COMPANY<br>G. H. Clamer, Second Vice-President and Secretary, 46 Richmond Street, Philadelphia, Pa. | 1902 |
| ALLEN, FRANCIS B<br>Mechanical Engineer, Second Vice-President, Hartford Steam Boiler Inspection and Insurance Company, Hartford, Conn. | 1902 |
| AMERICAN BRIDGE COMPANY<br>C. C. Schneider, Vice-President, Pencoyd, Pa. | 1902 |
| AMERICAN FOUNDRYMEN'S ASSOCIATION<br>Richard Moldenke, Secretary, P. O. Box 432, New York, N. Y. | 1898 |
| AMERICAN IRON AND STEEL MANUFACTURING COMPANY<br>H. M. Sternbergh, Vice-President, Reading, Pa. | 1902 |
| AMERICAN SOCIETY OF MECHANICAL ENGINEERS<br>F. R. Hutton, Secretary, 12 West Thirty-first Street, New York, N. Y. | 1897 |

American Steel and Wire Company..... 1900
F. H. Daniels, Chief Engineer, Worcester, Mass.

Anderson, Frederick Paul ..... 1899
Dean of School of Mechanical Engineering, State College of Kentucky, Lexington, Ky.

Anderson, J. W..... 1896
Carbon Steel Company, Pittsburg, Pa.

Anderson, R. Wilson..... 1897
Superintendent Open-Hearth Plant, Carbon Steel Company, Pittsburg, Pa.

Arnold, Charles E..... 1902
Chief Chemist, Dominion Iron and Steel Company, Sydney, C. B., Canada.

Bailey, J. B..... 1902
Treasurer and General Manager Central Iron and Steel Company, Harrisburg, Pa.

Barbour, Frank A..... 1898
Civil Engineer, Snow & Barbour, 1121 Tremont Building, Boston, Mass.

Bartol, George E..... 1902
President Dexter Portland Cement Company, 232 The Bourse, Philadelphia, Pa.

Beckett, James A..... 1903
General Superintendent Walter A. Wood Mower and Reaper Company, Hoosick Falls, N. Y.

Benneville, James S. de..... 1896
Analytical Chemist, University Club, 1510 Walnut Street, Philadelphia, Pa.

Berger, Bernt..... 1902
Civil Engineer, Assistant Engineer to Theodore Cooper, 35 Broadway, New York, N. Y.

Bethlehem Steel Company..... 1898
Albert Ladd Colby, Metallurgical Engineer, South Bethlehem, Pa.

BISSELL, GEORGE W. ..... 1896
Professor Mechanical Engineering, Iowa State College, Ames, Iowa.

BLACK, W. P. ..... 1896
Address unknown.

BLISS, COLLINS P. ..... 1902
Professor of Mechanical Engineering and Director of Testing Laboratory, New York University, University Heights, New York, N. Y.

BOLLER & HODGE ..... 1903
Consulting Engineers, 1 Nassau Street, New York, N. Y.

BONZANO, A. ..... 1902
President Bonzano Rail-Joint Company, 331 South Eighteenth Street, Philadelphia, Pa.

BOOTH, GARRETT & BLAIR ..... 1896
Engineers and Chemists, 406 Locust Street, Philadelphia, Pa.

BOYNTON, C. W. ..... 1902
Cement Inspector, Baltimore & Ohio Railroad, Wheeling, W. Va.

BRADY, T. J. ..... 1902
President Coplay Cement Manufacturing Company, Townsend Building, New York, N. Y.

BRAINE, L. F. ..... 1900
General Manager Continuous Rail Joint Company, Newark, N. J.

BRAMWELL, JOSEPH W. ..... 1898
Falkenau-Sinclair Machine Company, 109 North Twenty-second Street, Philadelphia, Pa.

BROADHURST, W. H. ..... 1899
Chemist, Department of Public Works, Municipal Building, Brooklyn, N. Y.

Brown, William ..... 1899
Superintendent of Steel Plant, Lukens Iron and Steel Company, 64 First Avenue, Coatesville, Pa.

Burdett, F. A. ..... 1902
Civil Engineer, 3 East Thirty-third Street, New York, N. Y.

Burr, William H. ..... 1899
Professor of Civil Engineering, Columbia University, New York, N. Y.

Bury, Edmund ..... 1901
Herron & Bury Manufacturing Company, Erie, Pa.

Cambier, Jacob ..... 1902
Chemist Colorado Fuel and Iron Company, 910 Spruce Street, Pueblo, Colo.

Cambria Steel Company ..... 1899
George E. Thackray, Structural Engineer, Johnstown, Pa.

Campbell, H. H. ..... 1896
Superintendent and General Manager, The Pennsylvania Steel Company, Steelton, Pa.

Capp, John A. ..... 1898
Engineer, Testing Laboratory, General Electric Company, Schenectady, N. Y.

Carnegie Steel Company ..... 1898
John McLeod, Assistant to President, Pittsburg, Pa.

Carpenter, Louis G. ..... 1902
Professor of Civil and Irrigation Engineering, and Director of Experiment Station, Fort Collins, Colo.

Carpenter, Rolla C. ..... 1895
Professor Experimental Engineering, Cornell University, 31 Eddy Street, Ithaca, N. Y.

Carr, Lovell H. ..... 1902
General Sales Agent, the Edison Portland Cement Company, 71 Broadway, New York, N. Y.

CARTER, ROBERT A. ..... 1899
President Monongahela Iron and Steel Company, Box 215, Pittsburg, Pa.

CENTRAL IRON AND STEEL COMPANY ..... 1902
James B. Bailey, Treasurer and General Manager, Harrisburg, Pa.

CHURCHILL, CHARLES S. ..... 1900
Chief Engineer, Norfolk and Western Railway, Roanoke, Va.

CHRISTIE, JAMES (*Member Executive Committee*) .... 1898
Chief Mechanical Engineer, American Bridge Company, Pencoyd, Pa.

CLARK, F. H. ..... 1900
Mechanical Engineer, Chicago, Burlington & Quincy Railroad, 209 Adams Street, Chicago, Ill.

COFFIN, JOHN N. ..... 1901
Consulting Mechanical Engineer, 312 Perry Payne Building, Cleveland, Ohio.

COLBY, ALBERT LADD (*Member Executive Committee*) 1899
Metallurgical Engineer, Bethlehem Steel Company, South Bethlehem, Pa

COLBY, J. ALLEN ..... 1899
Inspecting Engineer, Witherspoon Building, Philadelphia, Pa.

COLORADO FUEL AND IRON COMPANY ..... 1900
C. S. Robinson, General Manager Iron Department, Denver, Colo.

CONDRON, T. L. ..... 1900
Resident Engineer, Pittsburg Testing Laboratory, 1750 Monadnock Building, Chicago, Ill.

CORTHELL, E. L. ..... 1899
Civil Engineer, 1 Nassau Street, New York, N. Y.

COSBY, SPENCER ..... 1902
Captain, Corps of Engineers, U. S. Engineer Office, Mobile, Ala.

Cruikshank, Barton ..... 1899
Cogswell Polytechnic College, Twenty-sixth and Folsom Streets, San Francisco, Cal

Davidson, George M ..... 1900
Engineer of Tests, Chicago and Northwestern Railroad, Chicago, Ill.

DeArmond, W. C ..... 1902
President Protectus Company, 1103 North American Building, Philadelphia, Pa.

Deans, John Sterling ..... 1899
Chief Engineer Phoenix Bridge Company, Phoenixville, Pa.

Derleth, Charles, Jr. ..... 1902
Professor of Civil Engineering, State University, Colorado, Boulder, Colo.

Diller, H. E. ..... 1903
Chief Chemist, Western Electric Company, Chicago, Ill.

Dixon Crucible Company, Joseph ..... 1902
Malcolm McNaughton, Superintendent Paint and Lubricating Department, Jersey City, N. J.

Doble, William A ..... 1901
Mechanical Engineer, President Abner Doble Company, 200 Fremont Street, San Francisco, Cal.

Dow, A. W ..... 1898
Inspector of Asphalts and Cements, District of Columbia, Washington, D. C.

Drown, Thomas M ..... 1899
President Lehigh University, South Bethlehem, Pa.

DuComb, W. C., Jr ..... 1902
Mechanical Engineer, Engineer of Tests, 1424 North Ninth Street, Philadelphia, Pa.

DUDLEY, CHARLES B. (*President*) ..... 1896
Chemist, Pennsylvania Railroad, Altoona, Pa.

DUDLEY, P. H. ..... 1902
Consulting Engineer, 80 Pine Street, New York, N. Y.

DUFOUR, F. O. ..... 1901
Professor of Civil Engineering, University of Cincinnati, Cincinnati, Ohio.

DUGAN, CHARLES M., JR. ..... 1900
Chemist, 2139 North Seventh Street, Philadelphia, Pa.

DUMARY, L. HENRY ..... 1902
President The Helderberg Cement Company, 38 State Street, Albany, N. Y.

DUNBAR, W. O. ..... 1902
Assistant Engineer, Pennsylvania Railroad Testing Department, Altoona, Pa.

EASBY, M. WARD ..... 1902
Consulting Engineer, 909 Crozer Building, Philadelphia, Pa.

EDWARDS, WARRICK R. ..... 1902
Assistant Engineer of Bridges and Buildings, Baltimore and Ohio Railroad, Mt. Royal Station, Baltimore, Md.

ELDRIDGE, GEORGE H. ..... 1903
Geologist, United States Geological Survey, Washington, D. C.

ELY, THEODORE N. ..... 1896
Chief of Motive Power, Pennsylvania Railroad, Broad Street Station, Philadelphia, Pa.

EMRIE, ALMON ..... 1901
Superintendent Ingersoll-Sergeant Drill Company, Easton. Pa.

ENGINEERING RECORD ..... 1898
Henry C. Meyer, Jr., Editorial Staff, 100 William Street, New York, N. Y.

Esterline, J. Walter ..... 1903
Assistant Professor of Electrical Engineering, Purdue University, Lafayette, Ind.

Estrada, E. D ..... 1896
Engineer, Department of Public Works, Pinar del Rio, Cuba

Faculty Applied Science, McGill University ..... 1903
Henry T. Bovey, Dean, Montreal, Can.

Fahrig, Ernst ..... 1900
Chief of Laboratories, Philadelphia Commercial Museums, Philadelphia, Pa.

Falkenau, A ..... 1902
Engineer and Machinist, 4602 Kingsessing Avenue, Philadelphia, Pa.

Fay, Henry ..... 1902
Assistant Professor Analytical Chemistry and Metallography, Massachusetts Institute of Technology, Boston, Mass.

Field, Herbert E ..... 1902
Metallurgist, Farrel Foundry and Machine Company, Ansonia, Conn.

Flagg, Stanley G., Jr ..... 1899
Stanley G. Flagg & Co., Nineteenth Street and Pennsylvania Avenue, Philadelphia, Pa.

Forrest, C. N ..... 1901
Chemist and Inspector, Long Island Railroad, Long Island City, N. Y.

Franklin Institute ..... 1898
William H. Wahl, Secretary, 15 South Seventh Street, Philadelphia, Pa.

Freeman, S. E ..... 1901
Consulting Mechanical Engineer, 4052 Olive Street, St. Louis, Mo.

Galbraith, J ..... 1903
Principal School of Practical Science, Toronto, Can.

ROWLAND, JOHN F., JR. ..... 1898
Engineer International Navigation Company, Philadelphia, Pa.

SABIN, A. H. ..... 1898
Chemist, 45 Broadway, New York, N. Y.

SABIN, L. C. ..... 1902
United States Assistant Engineer, Detroit, Mich.

SAGUE, J. E. ..... 1902
Mechanical Engineer, American Locomotive Company, 25 Broad Street, New York, N. Y.

SAUNDERS, WALTER M. ..... 1902
Analytical and Consulting Chemist, 184 Whittier Avenue, Providence, R. I.

SAUVEUR, ALBERT ..... 1896
Assistant Professor of Metallurgy, Harvard University; Manager Boston Testing Laboratories, 446 Tremont Street, Boston, Mass.

SCARBOROUGH, F. W. ..... 1903
Engineer, Maintenance of Way, Chesapeake and Ohio Railway, Richmond, Va.

SCHAFFER, HERBERT A. ..... 1898
Chemist, Vulcanite Portland Cement Company, Phillipsburg, N. J.

SCHNEIDER, HERMAN ..... 1900
Instructor in Civil Engineering, Lehigh University, South Bethlehem, Pa.

SCHUERMAN, W. H. ..... 1902
Dean of Engineering Department and Professor of Civil Engineering, Vanderbilt University, Nashville, Tenn.

SCHUMANN, FRANCIS ..... 1898
Civil and Mechanical Engineer, 7000 Tulip Street, Tacony, Philadelphia, Pa.

SCOTT, W. G. ..... 1902
Chemist, J. I. Case Threshing Machine Company, 1109 Park Avenue, Racine, Wis.

Seaman, Harry J. ..... 1898
Superintendent Atlas Cement Company, Catasauqua, Pa.

Seaman, Henry B. ..... 1902
Civil Engineer, 40 Wall Street, New York, N. Y.

Shankland, E. C. & R. M. ..... 1902
Civil Engineers, 1106 Rookery, Chicago, Ill.

Shelby Steel Tube Company ..... 1902
J. H. Nicholson, Assistant to First Vice-President, The Frick Building, Pittsburg, Pa.

Sherman, C. W. ..... 1903
General Manager Pennsylvania Malleable Company, Central Car Wheel Company, Frick Building, Pittsburg, Pa.

Sherrerd, Morris R. ..... 1903
Engineer and Superintendent, Department Water, City of Newark, 128 Halsey Street, Newark, N. J.

Sherwin-Williams Company, The ..... 1902
Paint and Varnish Makers, E. C. Holton, Chemist in Chief, 100 Canal Street, Cleveland, O.

Shimer, Porter W. ..... 1899
Chemist and Metallurgist, Easton, Pa.

Shuman, Jesse J. ..... 1902
Chief Inspector, Testing Department, Jones & Laughlin Steel Company, 837 Heberton Avenue, Pittsburg, Pa.

Slocum, A. W. ..... 1903
General Superintendent Keystone Car Wheel Company, Pittsburg, Pa.

Smith, H. E. ..... 1902
Chemist, The Lake Shore & Michigan Southern Railway Company, Collinwood, Ohio.

Snow, J. P. ..... 1902
Bridge Engineer, Boston and Maine Railroad, Boston, Mass.

IRELAND, GUY L. ..... 1898
Mechanical Engineer, 314 Crown Street, New Haven, Conn.

IRON TRADE REVIEW, THE ..... 1902
A. I. Findlay, Editor, Cleveland, Ohio.

JARECKI, ALEXANDER ..... 1896
Superintendent Jarecki Manufacturing Company, Limited, Erie, Pa.

JENKINS, JOHN ..... 1897
General Manager Milton Iron Company, Milton, Pa.

JEWETT, J. Y. ..... 1900
Cement Inspector, Metropolitan Water Board of Massachusetts, Clinton, Mass.

JOB, ROBERT ..... 1900
Chemist, Philadelphia and Reading Railroad, Reading, Pa.

JOHNSON, WALLACE C. ..... 1900
Consulting Engineer, Niagara Falls, N. Y.

JONES & LAUGHLINS, LIMITED ..... 1902
Steel Manufacturers; Willis L. King, Vice-Chairman, Pittsburg, Pa.

KEEP, WILLIAM J. ..... 1896
Superintendent Michigan Stove Company, 753 Jefferson Avenue, Detroit, Mich.

KEMP, JAMES F. ..... 1898
Professor of Geology, School of Mines, Columbia University, New York, N. Y.

KENNEDY, FRANK G., JR. ..... 1899
Superintendent Logan Iron and Steel Company, Burnham, Mifflin County, Pa.

KENNEDY, JULIAN ..... 1899
Consulting Engineer, Latrobe Company, Pittsburg, Pa.

KENNICOTT, CASS L. ..... 1902
General Manager Kennicott Water Softener Company, 3567 Butler Street, Chicago, Ill.

Kent, William ..... 1902
Consulting Engineer, 220 Broadway, N. Y.

King, Willis L..... 1899
Vice-Chairman Jones & Laughlins, Limited, Pittsburg, Pa.

Kinkead, J. A..... 1899
Engineer of Tests, American Locomotive Company, Schenectady, N. Y.

Kirchoff, C..... 1902
Editor *The Iron Age*, 232 William Street, New York, N. Y.

Koehl, James C..... 1901
Engineer Kirksville Foundry and Machine Company, Kirksville, Adair County, Mo.

Kreuzpointner, Paul ..... 1896
Engineer of Tests, Pennsylvania Railroad, Altoona, Pa.

Landreth, Olin H..... 1896
Professor Engineering, Union College, Schenectady, N. Y.

Lanza, Gaetano..... 1899
Professor Theoretical and Applied Mechanics, in charge Mechanical Engineering Department, Massachusetts Institute of Technology, Boston, Mass.

Lathbury & Spackman..... 1898
Chemical and Physical Laboratories, 1619 Filbert Street, Philadelphia, Pa.

Latrobe Steel Company ..... 1898
Marriot C. Smyth, President, 1200 Girard Building, Philadelphia, Pa.

Lawrence, William H..... 1901
Assistant Professor of Architecture, Massachusetts Institute of Technology, Boston, Mass.

LESLEY, R. W. (*Vice-President*) ..... 1898
President American Cement Company, 22 South Fifteenth Street, Philadelphia, Pa.

LEWIS, FREDERICK H. ..... 1903
Manager Virginia Portland Cement Company, Craigsville, Va.

LEWIS, GEORGE T. ..... 1902
Secretary and Treasurer Monongahela Iron and Steel Company, Box 215, Pittsburg, Pa.

LINDENTHAL, GUSTAV. ..... 1896
Commissioner of Bridges, City of New York, Park Row Building, New York, N. Y.

LINTON, HARVEY ..... 1902
City Engineer, Altoona, Pa.

LOBER, J. B. ..... 1902
Vice-President Vulcanite Portland Cement Company, Real Estate Trust Building, Philadelphia, Pa.

LOCKARD, CHAS. A. ..... 1902
Manager Empire Portland Cement Company, Warners, N. Y.

LORDLY, HENRY ROBERTSON ..... 1903
Engineer in Charge, Lachine Canal, Royal Insurance Building, Montreal, Canada.

LOUDON, ARCH. M. ..... 1903
President Associated Foundry Foremen, 544 West One Hundred and Twenty-sixth St., New York, N.Y.

LOVELL, EARL B. ..... 1902
Adjunct Professor of Civil Engineering, Columbia University, 235 West One Hundred and Second Street, New York, N. Y.

LOWE BROTHERS ..... 1899
Paint and Color Makers; Huston Lowe, Vice-President, Dayton, Ohio.

LOWETH, CHARLES F. ..... 1900
Engineer and Superintendent of Bridges and Buildings, Chicago, Milwaukee and St. Paul Railway, 1100 Old Colony Building, Chicago, Ill.

Lukens Iron and Steel Company..... 1902
Charles L. Huston, Vice-President, Coatesville, Pa

Lundteigen, Andreas ..... 1898
Chemist, Union City, Mich.

Lynch, T. D..... 1902
Inspector of Material, Westinghouse Electrical Manufacturing Company, East Pittsburg, Pa.

MacPherran, R. S..... 1902
Chemist, Allis-Chalmers Company, Milwaukee, Wis.

McCauley, H. K..... 1896
Secretary and Treasurer Altoona Iron Company, Altoona, Pa.

McCormick Division, International Harvester Company ..... 1902
F. A. Flather, Superintendent, Blue Island and Western Avenues, Chicago, Ill.

McKenna, Charles F..... 1896
Chemist, 221 Pearl Street, New York, N. Y.

Maclay, William W..... 1895
President Glens Falls Portland Cement Company, Glens Falls, N. Y.

Major, Charles ..... 1902
President A. & P. Roberts Company, Manager Pencoyd Iron Works, Pencoyd, Pa.

MARBURG, EDGAR (*Secretary-Treasurer*)..... 1898
Professor of Civil Engineering, University of Pennsylvania, Philadelphia, Pa.

March, H. J..... 1899
Civil Engineer, 13 City Hall, Buffalo, N. Y.

Martin, Simon S..... 1902
Superintendent Steel Department, Maryland Steel Company, Sparrows Point, Md.

Matcham, Chas. A..... 1902
Manager Lehigh Portland Cement Company, Allentown, Pa.

MATHEWS JOHN A. ..... 1903
Metallurgist, Experimental Department, Crucible Steel Company of America, Syracuse, N. Y.

MEADE, RICHARD K. ..... 1899
Chemist, The Edison Portland Cement Company, Stewartsville, N. J.

MEIER, E. D. ..... 1902
President and Chief Engineer Herne Safety Boiler Company, 11 Broadway, New York, N. Y.

MERRIMAN, MANSFIELD (*Past-President*) ..... 1895
Professor of Civil Engineering, Lehigh University, South Bethlehem, Pa.

MILLS, CHARLES M. ..... 1900
Principal Assistant Engineer, Subway and Elevated Railroad Construction, Rapid Transit Company, Philadelphia, Pa.

MITCHELL, WILLIAM H. ..... 1901
Engineer, 100 Broadway, New York, N. Y.

MOLDENKE, RICHARD G. G ..... 1896
Metallurgist, Consulting Engineer, Box 432, New York, N. Y.

MOORE, WILLIAM HARLEY ..... 1902
Engineer of Bridges, New York, New Haven and Hartford Railroad, New Haven, Conn.

MORISON, GEORGE S. ..... 1896
Consulting Engineer, 49 Wall Street, New York, N. Y.

MUESER, WILLIAM ..... 1899
Civil Engineer, Member Concrete-Steel Engineering Company, 13–21 Park Row Building, New York, N. Y.

MUTUAL BOILER INSURANCE COMPANY ..... 1899
31 Milk Street, Boston, Mass.

NATIONAL TUBE COMPANY ..... 1900
Taylor Allderdice, Assistant to First Vice-President, Conestago Building, Pittsburg, Pa.

NEALE, JAMES ..... 1896
Secretary Wayne Iron Works, Pittsburg, Pa.

Neff, F. H. ..... 1902
Professor of Civil Engineering, Case School of Applied Science, Cleveland, O.

Newberry, Spencer B. ..... 1898
Manager Sandusky Portland Cement Company, Sandusky, Ohio.

New York Air Brake Company, The ..... 1902
R. C. Augur, Mechanical Engineer, Watertown, N. Y.

Noble, Alfred ..... 1903
Chief Engineer, East River Section, Pennsylvania, New York and Long Island Railroad, 20 West Thirty-fourth Street, New York, N. Y.

Norris, George L. ..... 1902
Chemist Standard Steel Works, Burnham, Pa.

Olsen, Tinius ..... 1898
Tinius Olsen & Company, Testing Machines, 500 North Twelfth Street, Philadelphia, Pa.

Onderdonk, J. R. ..... 1902
Engineer of Tests, Baltimore and Ohio Railroad, Mt. Clare, Baltimore, Md.

Orton, Edward, Jr. ..... 1902
Dean, College of Engineering, Ohio State University, and State Geologist of Ohio, Columbus, O.

Osborn Engineering Company, The ..... 1898
Frank C. Osborn, Cleveland, Ohio.

Ostrom, John N. ..... 1903
Bridge Engineer, 1525 Frick Building, Pittsburg, Pa.

Outerbridge, Alex. E., Jr. ..... 1902
Chemist and Metallurgist, 1600 Hamilton Street, Philadelphia, Pa.

Page, Logan Waller ..... 1903
Chief of Road Material Laboratory, United States Department of Agriculture, Washington, D. C.

PARKHURST, H. W. ..... 1902
Engineer of Bridges and Buildings, Illinois Central Lines, 1 Park Row, Chicago, Ill.

PATTERSON, BEN ..... 1902
President Patterson-Sargent Company, Cleveland, Ohio.

PEARSON, H. C. ..... 1902
Editor *India Rubber World*, 150 Nassau Street, New York, N. Y.

PEASE, F. N. ..... 1896
Assistant Chemist, Pennsylvania Railroad, Altoona, Pa.

PECKHAM, S. F. ..... 1903
Chemist to the Commissioners of Accounts, New York City, Room 104, 280 Broadway, New York, N. Y.

PENNSYLVANIA STEEL COMPANY, THE ..... 1902
H. H. Campbell, Superintendent and General Manager, Steelton, Pa.

PHILLIPS, WILLIAM BATTLE ..... 1903
Mining Engineer and Metallurgist, Director University of Texas Mineral Survey, Austin, Tex.

POLK, W. A. ..... 1902
Sales Agent, The Patterson-Sargent Company, 42 Hudson Street, New York, N. Y.

PORTER, JAMES MADISON ..... 1898
Professor of Civil Engineering, Lafayette College, Easton, Pa.

POWERS, W. A. ..... 1903
Chief Chemist, Atchison, Topeka and Santa Fé Railroad, Topeka, Kan.

RAILROAD GAZETTE ..... 1898
H. G. Prout, Editor, 32 Park Place, New York, N. Y.

RAILWAY AND ENGINEERING REVIEW ..... 1902
W. M. Camp, Editor, Chicago, Ill.

Randolph, Lingan S. ..... 1896
Professor of Mechanical Engineering, Virginia Polytechnic Institute, Blacksburg, Va.

Reading Iron Company ..... 1902
David Thomas, Superintendent Montaur Rolling Mills, Danville, Pa.

Reid, David ..... 1902
Superintendent of Foundry, American Machine Company, 615 Broadway, Pawtucket, R. I.

Rice, Francis S. ..... 1898
Structural Engineer, Aspinwall, Pa.

Richards, Joseph T. ..... 1900
Engineer Maintenance of Way, Pennsylvania Railroad, Broad Street Station, Philadelphia, Pa.

Richards, Joseph W. ..... 1902
Assistant Professor of Metallurgy, Lehigh University, Bethlehem, Pa.

Richards, Robert H. ..... 1902
Professor of Mining Engineering and Metallurgy, Massachusetts Institute of Technology, Boston, Mass.

Richardson, Clifford ..... 1896
Asphalt Expert, New York Testing Laboratory, Long Island City, N. Y.

Richter, A. W. ..... 1900
Assistant Professor of Experimental Engineering, 428 Murray Street, Madison, Wis.

Riegner, W. B. ..... 1902
Engineer of Bridges, Philadelphia and Reading Railway, Reading Terminal, Philadelphia.

Riehlé, Frederick A. ..... 1898
Riehlé Bros. Testing Machine Company, 1424 North Ninth Street, Philadelphia, Pa.

Roebling's (John A.) Sons Company ..... 1900
J. H. Janeway, Jr., Mechanical Engineer, Trenton, N. J.

ROWLAND, JOHN F., JR. ..... 1898
Engineer International Navigation Company, Philadelphia, Pa.

SABIN, A. H. ..... 1898
Chemist, 45 Broadway, New York, N. Y.

SABIN, L. C. ..... 1902
United States Assistant Engineer, Detroit, Mich.

SAGUE, J. E. ..... 1902
Mechanical Engineer, American Locomotive Company, 25 Broad Street, New York, N. Y.

SAUNDERS, WALTER M. ..... 1902
Analytical and Consulting Chemist, 184 Whittier Avenue, Providence, R. I.

SAUVEUR, ALBERT ..... 1896
Assistant Professor of Metallurgy, Harvard University; Manager Boston Testing Laboratories, 446 Tremont Street, Boston, Mass.

SCARBOROUGH, F. W. ..... 1903
Engineer, Maintenance of Way, Chesapeake and Ohio Railway, Richmond, Va.

SCHAFFER, HERBERT A. ..... 1898
Chemist, Vulcanite Portland Cement Company, Phillipsburg, N. J.

SCHNEIDER, HERMAN ..... 1900
Instructor in Civil Engineering, Lehigh University, South Bethlehem, Pa.

SCHUERMAN, W. H. ..... 1902
Dean of Engineering Department and Professor of Civil Engineering, Vanderbilt University, Nashville, Tenn.

SCHUMANN, FRANCIS ..... 1898
Civil and Mechanical Engineer, 7000 Tulip Street, Tacony, Philadelphia, Pa.

SCOTT, W. G. ..... 1902
Chemist, J. I. Case Threshing Machine Company, 1109 Park Avenue, Racine, Wis.

Seaman, Harry J. ..... 1898
Superintendent Atlas Cement Company, Catasauqua, Pa.

Seaman, Henry B. ..... 1902
Civil Engineer, 40 Wall Street, New York, N. Y.

Shankland, E. C. & R. M. ..... 1902
Civil Engineers, 1106 Rookery, Chicago, Ill.

Shelby Steel Tube Company ..... 1902
J. H. Nicholson, Assistant to First Vice-President, The Frick Building, Pittsburg, Pa.

Sherman, C. W. ..... 1903
General Manager Pennsylvania Malleable Company, Central Car Wheel Company, Frick Building, Pittsburg, Pa.

Sherrerd, Morris R. ..... 1903
Engineer and Superintendent, Department Water, City of Newark, 128 Halsey Street, Newark, N. J.

Sherwin-Williams Company, The ..... 1902
Paint and Varnish Makers, E. C. Holton, Chemist in Chief, 100 Canal Street, Cleveland, O.

Shimer, Porter W. ..... 1899
Chemist and Metallurgist, Easton, Pa.

Shuman, Jesse J. ..... 1902
Chief Inspector, Testing Department, Jones & Laughlin Steel Company, 837 Heberton Avenue, Pittsburg, Pa.

Slocum, A. W. ..... 1903
General Superintendent Keystone Car Wheel Company, Pittsburg, Pa.

Smith, H. E. ..... 1902
Chemist, The Lake Shore & Michigan Southern Railway Company, Collinwood, Ohio.

Snow, J. P. ..... 1902
Bridge Engineer, Boston and Maine Railroad, Boston, Mass.

SOUTHER, HENRY ..... 1903
Consulting Metallurgical Engineer, State Chemist, 440 Capitol Avenue, Hartford, Conn.

SPANGLER, H. W. ..... 1902
Professor of Mechanical Engineering, University of Pennsylvania, Philadelphia, Pa.

SPERRY, W. L. ..... 1901
Superintendent and Manager, The Cumberland Hydraulic Cement and Manufacturing Company, P. O. Box 234, Cumberland, Md.

STANDARD STEEL WORKS ..... 1902
A. A. Stevenson, Assistant Superintendent, Burnham, Pa.

STAUFFER, DAVID MCN ..... 1896
Civil Engineer, Editor *Engineering News*, 220 Broadway, New York, N. Y.

STEINMAN, A. J. ..... 1899
Chairman Pennsylvania Iron Company, Limited, Lancaster, Pa.

STERNBERGH, H. M. ..... 1902
Vice-President American Iron and Steel Manufacturing Company, Reading, Pa.

STEVENSON, A. A. ..... 1896
Assistant Superintendent Standard Steel Works, Burnham, Mifflin County, Pa.

STILLMAN, THOMAS B. ..... 1899
Chemist, Stevens Institute of Technology, Hoboken, N. J.

STOREY, W. B., JR. ..... 1903
Chief Engineer, Atchison, Topeka and Santa Fé Railway Company, 1015 Van Buren Street, Topeka, Kan,

STOUGHTON, BRADLEY ..... 1902
Tutor in Metallurgy, and Consulting Metallurgist, Columbia University, New York, N. Y.

Strobel, Charles L. . . . . 1896
Consulting Engineer, 1744 Monadnock Block, Chicago, Ill.

Swain, George F. . . . . 1896
Professor of Civil Engineering, Massachusetts Institute of Technology, Boston, Mass.

Talbot, Arthur N. . . . . 1898
Professor of Municipal and Sanitary Engineering, University of Illinois, Urbana, Ill.

Talbot, Henry P. . . . . 1902
Professor of Inorganic and Analytical Chemistry, Massachusetts Institute of Technology, Boston, Mass.

Taylor, William Purves . . . . . 1900
Engineer in Charge, Testing Laboratory, 418 City Hall, Philadelphia, Pa.

Technischer Verein Brooklyn . . . . . 1896
W. Schad, Secretary, 105 North Eleventh Street, Brooklyn, N. Y.

Technischer Verein New York . . . . . 1896
Carl Kaelble, Secretary, Room 705, 290 Broadway, New York, N. Y.

Technischer Verein Philadelphia . . . . . 1896
534 North Fourth Street, Philadelphia, Pa.

Technischer Verein Pittsburg . . . . . 1896
E. F. Harder, Secretary, 321 Savannah Avenue, Pittsburg, Pa.

Technischer Verein Washington . . . . . 1896
Paul Bausch, Corresponding Secretary, 3418 Brown Street, N. W., Washington, D. C.

Thacher, Edwin . . . . . 1902
Consulting Engineer, Member Concrete-Steel Engineering Company, Park Row Building, New York, N. Y.

Thackray, George E. . . . . 1899
Structural Engineer, Cambria Steel Company, Johnstown, Pa.

THOMAS, DAVID ..... 1900
Superintendent Montaur Rolling Mills, Reading Iron Company, Danville, Pa.

THURSTON, ROBERT H ..... 1896
Director Sibley College, Cornell University, Ithaca, N. Y.

TIPPETT AND WOOD ..... 1900
Plate Iron and Steel Workers, Phillipsburg, N. J.

TOUCEDA, ENRIQUE ..... 1903
Chemist and Metallurgist, 51 State Street, Albany, N. Y.

TREAT, R. B. ..... 1903
Electrical Engineer, Crocker-Wheeler Company, Ampere, N. J.

TURNEAURE, F. E. ..... 1902
Professor of Bridge and Sanitary Engineering, University of Wisconsin, Madison, Wis.

UMSTEAD, C. H ..... 1902
Civil Engineer, 30 Austin Street, Portsmouth, N. H.

VAN ORNUM, J. L. ..... 1902
Professor of Civil Engineering, Washington University, St. Louis, Mo.

VOGT, A. S ..... 1896
Mechanical Engineer, Pennsylvania Railroad, Altoona, Pa.

VOORHEES, S. S ..... 1902
Engineer of Tests, Treasury Department, Washington, D. C.

WADDELL, J. A. L ..... 1896
Consulting Civil Engineer, Kansas City, Mo.

WAGNER, SAMUEL TOBIAS ..... 1899
Assistant Engineer, Philadelphia and Reading Railroad, Reading Terminal, Philadelphia, Pa.

WALKER, JOSEPH F ..... 1902
Chemist, The Protectus Company, Bridgeport, Pa.

Walker, W. H. ..... 1903
Associate Professor of Industrial Chemistry, Massachusetts Institute of Technology, Boston, Mass.

Webster, George S. ..... 1900
Chief Engineer and Surveyor, Bureau of Surveys, 418 City Hall, Philadelphia, Pa.

Webster, William R. ..... 1898
Civil Engineer, 411 Walnut Street, Philadelphia, Pa.

West, Thomas D. ..... 1897
Foundry Expert, Sharpsville, Pa.

Whitehead, J. W., Jr. ..... 1900
National Lead Company, 1510 State Street, Chicago, Ill.

Whitney, Asa W. ..... 1902
Foundry Specialist, Lynchburg Plow and Foundry Company, Lynchburg, Va.

Whitney, Willis R. ..... 1902
Associate Professor of Chemistry, Massachusetts Institute of Technology, Boston, Mass.

Wickhorst, Max H. ..... 1898
Engineer of Tests, Chicago, Burlington and Quincy Railroad, Aurora, Ill.

Wilgus, William J. ..... 1902
Chief Engineer, New York Central and Hudson River Railroad, Grand Central Station, New York, N. Y.

Wilhelm Company, The A. ..... 1902
Paint Makers, Reading, Pa.

Wille, H. V. ..... 1898
Engineer of Tests, Baldwin Locomotive Works, 500 North Broad Street, Philadelphia, Pa.

Wing, Charles B. ..... 1900
Professor of Structural Engineering, Stanford University, Cal.

Wood Company, R. D., Founders ..... 1902
Walter Wood, 400 Chestnut Street, Philadelphia, Pa.

WOOD, WALTER ..... 1900
Cast Iron Pipe Manufacturer, R. D. Wood Company, 400 Chestnut Street, Philadelphia, Pa.

WOOLSON, IRA H ..... 1900
Instructor in Mechanical Engineering, Columbia University, New York, N. Y.

WYCKOFF, CHARLES, JR. ..... 1902
Assistant in Civil Engineering, Columbia University, 185 Penn Street, Brooklyn, N. Y.

ZEHNDER, C. H. ..... 1903
Manager, Rogers, Brown & Warren, Bullitt Building, Philadelphia, Pa.

## GEOGRAPHICAL DISTRIBUTION OF MEMBERS.

| | | | | | |
|---|---|---|---|---|---|
| Alabama | 1 | Maryland | 5 | Rhode Island | 2 |
| California | 3 | Massachusetts | 16 | Tennessee | 1 |
| Colorado | 4 | Michigan | 3 | Texas | 1 |
| Connecticut | 7 | Missouri | 4 | Virginia | 5 |
| Dist. of Columbia | 5 | Nebraska | 1 | West Virginia | 1 |
| Illinois | 17 | New Hampshire | 1 | Wisconsin | 4 |
| Indiana | 4 | New Jersey | 10 | Canada | 4 |
| Iowa | 1 | New York | 60 | Cuba | 1 |
| Kansas | 2 | Ohio | 10 | Addresses unknown | 2 |
| Kentucky | 1 | Pennsylvania | 114 | | |

Total Membership ..... 290

## DECEASED MEMBERS.

| Name. | Date of Membership. | Date of Death. |
|---|---|---|
| CHARLES JARECKI | 1896 | January 26, 1901 |
| J. B. JOHNSON | 1899 | June 23, 1902. |
| G. M. MCCAULEY | 1898 | May 25, 1901. |
| HENRY MORTON | 1896 | May 9, 1902. |

# PAST OFFICERS.

NOTE.—The Society, from its organization in 1898 till its incorporation under its present name in 1902, was designated the American Section of the International Association for Testing Materials.

The officers and members of the Executive Committee during this four-year period were as follows:

CHAIRMEN:

MANSFIELD MERRIMAN, 1898–1900.

HENRY M. HOWE, 1900–1902.

VICE-CHAIRMEN:

HENRY M. HOWE, 1898–1900.

CHARLES B. DUDLEY, 1900–1902.

SECRETARIES:

RICHARD L. HUMPHREY, 1898–1900.

J. M. PORTER, 1900–1902.

TREASURERS:

PAUL KREUZPOINTNER, 1898–1900.

R. W. LESLEY, 1900–1902.

MEMBERS OF EXECUTIVE COMMITTEE:

GUS. C. HENNING, 1898–1900.

ALBERT LADD COLBY, 1900–1902.

## ANNUAL REPORT OF THE EXECUTIVE COMMITTEE.

Since the Fourth Annual Meeting of the American Section in June, 1901, the Executive Committee has held four meetings, of which one was informal by reason of the absence of a quorum. An abstract of the Minutes of these meetings is appended to this report.

Upon careful consideration and in pursuance of repeated suggestions from the President of the International Association, the Executive Committee deems it advisable that the name of the organization be changed from the "American Section of the International Association for Testing Materials" to the "American Society for Testing Materials," and that, under this new name, application be made in due course for membership as a body in the International Association. Legal steps have already been taken by the Executive Committee for incorporation under the laws of Pennsylvania as the "American Society for Testing Materials." A copy of the Charter and the proposed By-Laws under the same are herewith submitted as an appendix to this report. The adoption of the Charter and By-Laws is unanimously recommended by the Executive Committee. The status and purposes of the Society will be in no way affected by such action. A prerequisite to membership in the Society will be then, as now, that the person, corporation or society seeking membership shall be a member of the International Association. Application for membership in the "American Society for Testing Materials" on the part of the American members of the "International Association for Testing Materials" will remain purely optional. Since the Statutes of the International Association do not recognize national sections as such, and to harmonize our status with that of other national societies who stand in like relation to the parent body, the above action is recommended. In all publications of the "American Society for Testing Materials" it is proposed that the name of the Society be followed by the words "Affiliated with the International Association for Testing Materials," to indicate our organic relation with that body.

*Publications.*—Since the last report three bulletins have been issued: Bulletin No. 26, containing the letter ballot on the Proposed Standard Specifications; Bulletin No. 27, containing the Minutes and Proceedings of the Fourth Annual Meeting, and announcing the result of the letter ballot on the Proposed Standard Specifications, and Bulletin No. 28, containing the Program for the Fifth Annual Meeting.

*Membership.*—At the last Annual Meeting the total membership of the American Section was 168. Since then the Section has sustained a loss of one member, in the death of Dr. Henry Morton, and of seven members by resignation. The applications for membership number fifteen, making a total membership at present of 175, or a net gain of seven for the year.

## REPORT OF THE TREASURER.

### RECEIPTS.

| | | | |
|---|---|---|---|
| October 14, 1901, J. M. Porter | | $ 25 00 | |
| April 15, 1902, J. M. Porter | | 318 70 | |
| June 9, 1902, Edgar Marburg | | 64 58 | |
| Total Receipts | | $408 28 | |
| Cash balance, June 18, 1901 | | 288 23 | |
| | | | $696 51 |

### EXPENDITURES.

| | | | |
|---|---|---|---|
| August 1, 1901, L. V. Tetmajer, dues | | $222 00 | |
| October 15, 1901, Chemical Publishing Company | | 37 75 | |
| April 15, 1902, " " " | | 65 75 | |
| April 15, 1902, William H. Hoskins, office supplies | | 2 08 | |
| April 15, 1902, J. M. Porter, Expenses Secretary and Publication Committee | | 55 42 | |
| April 15, 1902, L. J. Johnstone, stenographer | | 10 00 | |
| May 5, 1902, L. V. Tetmajer, dues | | 216 00 | |
| Total disbursements | | $609 00 | |
| Cash balance, June 9, 1902 | | 87 51 | |
| | | | $696 51 |

The unpaid bills on hand, June 9, 1902, amount to a total sum of $91.34.

The Report of the Treasurer shows the urgent necessity of measures toward the betterment of the financial condition of the Society. The annual dues for membership prescribed in the proposed new By-Laws are wholly inadequate for meeting

general operating expenses and the cost of publication. Nevertheless no further increase of dues is recommended for the present. It is confidently expected that the needed funds will be readily obtained, as in the past, through voluntary subscriptions on the part of individuals and corporations interested in the objects for which the Society stands.

Respectfully submitted,

EDGAR MARBURG, *Secretary.* HENRY M. HOWE, *Chairman.*

## APPENDIX A.

### ABSTRACT OF MINUTES OF THE EXECUTIVE COMMITTEE.

**Meeting, November 2, 1901.**—27 West Seventy-third street, New York, N. Y. Present: Messrs. Howe, Dudley and Merriman of the Executive Committee, and Mr. W. R. Webster, on invitation.

Professor Howe presented a report of the action of the Buda-Pesth Congress regarding the requests of the American Section: The Congress had resolved to appoint a Committee on Standard Specifications and Methods of Testing Cast Iron and Finished Castings, and also a Committee on Standard Specifications for Cements; it had given full authority to each country to elect its own Member of Council, but formal recognition of the American Section had not been secured.

Professor Porter's request, made at the meeting of the Executive Committee on June 29, 1901, to be relieved of the duties of the Secretaryship, was considered. It was thought that an effort should be made to secure the permanent services of a secretary who should receive a salary and devote much time to the work. A number of persons having qualifications for the office were suggested. The matter was finally referred to Chairman Howe and Vice-Chairman Dudley, with instructions to correspond with several of the persons suggested and to report at the next meeting of the Executive Committee.

**Meeting, February 15, 1902.**—27 West Seventy-third street, New York City. Present: Messrs. Howe and Dudley of the Executive Committee, and Professor Edgar Marburg on invitation.

In the absence of a quorum, the meeting adjourned after an informal discussion.

**Meeting, March 8, 1902.**—Engineers' Club, 374 Fifth Avenue, New York, N. Y. Present: Messrs. Howe, Dudley and Lesley of the Executive Committee, and Professor Edgar Marburg on invitation.

The resignation of Professor J. M. Porter from the office of Secretary was accepted with an expression of appreciation of his faithful and efficient services.

Professor Edgar Marburg was appointed Acting Secretary for the unexpired term.

The Acting Secretary was instructed to take legal advice towards the formal incorporation of the Society under the new title "American Society for Testing Materials," and to submit to a meeting of the Executive Committee, to be called for that purpose, the proposed Charter and By-Laws under the same. It was decided that the new By-Laws should be drawn up with a view of merging the offices of Secretary and Treasurer.

It was the formally expressed sense of the meeting that after the Charter shall be granted proper steps should be taken to join the International Association as a body.

The Acting Secretary was charged with the preparation of a preliminary draft of the program for the next Annual Meeting to be submitted at the next meeting of the Executive Committee.

The Acting Secretary was authorized to spend a sum not exceeding $25.00 per month for clerical assistance.

---

**Meeting, April 12, 1902.**—Engineers' Club of Philadelphia, 1122 Girard Street, Philadelphia, Pa. Present: Messrs. Howe, Lesley, Merriman and Marburg of the Executive Committee, and Messrs. Christie and Webster on invitation.

The Acting Secretary submitted the application for a Charter under the proposed new name of the "American Society for Testing Materials," which was formally approved with the understanding that if the Charter be accepted by the membership the words "Affiliated with the International Association for Testing Materials" shall appear on all printed matter, including bulletins, stationery, etc. immediately after the name of the Society.

The Acting Secretary submitted the draft of the proposed new By-Laws, which was considered by sections and, after changes and amendments, was adopted by sections and as a whole in the following form (see Appendix B).

The Acting Secretary was instructed to cast a ballot electing all present members of the American Section of the International Association for Testing Materials to the American Society for Testing Materials.

The Acting Secretary submitted the provisional program for the Fifth Annual Meeting to be held at Atlantic City, June 12, 13, 14.

This provisional program was approved and the Secretary given full discretion to modify it in any way that may seem necessary or expedient.

At the recommendation of Professor Merriman of the Publication Committee, it was decided that the Acting Secretary be instructed to have future bulletins issued by the Society copyrighted.

The report of the American Committee on "Bibliography of Im-

pact Tests" was referred to the Publication Committee with power to print.

With the approval of the Acting Secretary, it was decided that his title be changed from Acting Secretary to Secretary.

## APPENDIX B.

### CHARTER OF THE AMERICAN SOCIETY FOR TESTING MATERIALS.

*To the Honorable, the Judges of the Court of Common Pleas No. 2 in and for the City and County of Philadelphia: of March term, 1902, No. 2056:*

In compliance with the requirements of an Act of the General Assembly of the Commonwealth of Pennsylvania, entitled "An Act to Provide for the Incorporation and Regulation of Certain Corporations," approved the 29th day of April, A. D. one thousand eight hundred and seventy-four, and the supplements thereto, the undersigned, Henry M. Howe, Charles B. Dudley, Edgar Marburg, Robert W. Lesley, Mansfield Merriman, Albert Ladd Colby and William R. Webster, six of whom are citizens of Pennsylvania, having associated themselves together for the purposes hereinafter set forth, and desiring that they may be incorporated according to law, do hereby certify:

1. The name of the proposed corporation is the "American Society for Testing Materials."

2. The corporation is formed for the Promotion of Knowledge of the Materials of Engineering, and the Standardization of Specifications and the Methods of Testing.

3. The business of the said corporation is to be transacted in Philadelphia.

4. The said corporation is to exist perpetually.

5. The names and residences of the incorporators are as follows:

HENRY M. HOWE, 27 West Seventy-third Street, New York.
CHARLES B. DUDLEY, Altoona, Pa.
EDGAR MARBURG, 517 South Forty-first Street, Philadelphia.
ROBERT W. LESLEY, 22 South Fifteenth Street, Philadelphia.
MANSFIELD MERRIMAN, South Bethlehem, Pa.
ALBERT LADD COLBY, South Bethlehem, Pa.
WILLIAM R. WEBSTER, "The Bartram," Thirty-third and Chestnut Streets, Philadelphia.

6. The management of the said corporation shall be vested in an Executive Committee, consisting of six (6) members, viz: the Chairman, the Vice-Chairman, the Secretary, the Treasurer, and two other members of the corporation, and such other officers as the corporation may from time to time appoint.

7. The corporation has no capital stock, and the members thereof shall be composed of the subscribers and their associates and of such persons as may from time to time be admitted by vote in such manner and upon such requirements as may be prescribed by the By-Laws. The corporation shall nevertheless have power to exclude, expel or suspend members for just or legal cause, and in such legal manner as may be ordained and directed by the By-Laws.

8. The By-Laws of this corporation shall be admitted and taken to be its laws subordinate to the statute aforesaid; this Charter; Constitution and Laws of the Commonwealth of Pennsylvania, and the Constitution of the United States; they shall be altered and amended as provided for by the By-Laws themselves; and shall prescribe the powers and functions of the Executive Committee herein mentioned and those to be hereafter elected. The times and places of meetings of the Committee and this corporation; the number of members who shall constitute a quorum at the meetings of the corporation, and of the Committee; the qualifications and manner of electing members; the manner of electing officers; and the power and duties of such officers; and all other concerned and internal arrangements of the said corporation.

Witness our hands and seals this twenty-first day of March, A. D 1902.

[Signed] Edgar Marburg,
R. W. Lesley,
Wm. R. Webster,
Mansfield Merriman,
Albert Ladd Colby.

## PROPOSED BY-LAWS.

### ARTICLE I.

#### Members.

Section 1. Any person, corporation or technical society holding membership in the International Association for Testing Materials is eligible for membership.

Section 2. Any person, corporation or society can become a member of this Society and of the International Association for Testing Materials simultaneously upon being proposed by two members of this Society and being approved by its Executive Committee.

Section 3. Applications for membership and resignation from membership must be transmitted in writing to the Secretary.

### ARTICLE II.

#### Officers.

Section 1. The officers shall be a President, Vice-President, Secretary and Treasurer.

SECTION 2. The offices of Secretary and Treasurer shall be held by the same person.

SECTION 3. These officers shall be elected by letter-ballot at the Annual Meeting, and shall hold office for two years.

SECTION 4. The Executive Committee after the year 1901–1902, shall consist of these officers, the last past-President and three members, two being elected by letter-ballot at each Annual Meeting in the odd years and one at each Annual Meeting in the even years.

SECTION 5. The President shall be, *ex-officio*, the nominee for American Member of the Council of the International Association.

SECTION 6. The Secretary shall receive a salary to be fixed by the Executive Committee.

## ARTICLE III.

### MEETINGS.

SECTION 1. The Society shall meet annually. The time and place of each meeting shall be fixed by the Executive Committee.

SECTION 2. Special meetings may be called whenever the Executive Committee shall deem it necessary.

## ARTICLE IV.

### DUES.

SECTION 1. The fiscal year shall commence on the first of January, and all dues shall be payable in advance.

SECTION 2. The annual dues of each member shall be $3.00. Of this amount $1.50 shall be transmitted by the Secretary to the International Association for Testing Materials. The remainder shall be applied to the treasury of the Society

SECTION 3. Any member of the Society whose dues shall remain unpaid for the period of one year shall forfeit the privileges of membership. If he neglects to pay his dues within thirty days thereafter, and after notification from the Secretary, his name may be stricken from the roll of membership by the Executive Committee

## ARTICLE V.

### AMENDMENTS.

SECTION 1. Proposed Amendments to these By-Laws, signed by at least three members, must be presented in writing to the Executive Committee not later than the last regular meeting of the Executive Committee preceding the Annual Meeting. In the notices for this meeting the proposed Amendment shall be printed. At the Annual Meeting the proposed Amendment may be discussed and amended and may be passed to letter-ballot by a two-thirds vote of those present.

If two-thirds of the votes obtained by letter-ballot are in favor of the proposed Amendment, it shall be adopted.

SECTION 2. The Executive Committee is authorized to number the Articles and Sections of the By-Laws to correspond with any changes that may be made.

## REPORT OF AUDITING COMMITTEE.

PHILADELPHIA, PA., October 25, 1902.

*To the Executive Committee of the American Society for Testing Materials:*

We have examined the books and accounts of the Secretary and Treasurer, from December 29, 1900—the date of the last audit—to June 9, 1902, the date of the annual report of the Treasurer, and find the cash balance of $87.51 to be correct.

[Signed] JAMES CHRISTIE,
WM. R. WEBSTER,
*Auditing Committee.*

## PREVIOUS PUBLICATIONS.

### TABLE OF CONTENTS.

NOTE.—The Society, from its organization in 1898 till its incorporation under its present name in 1902, was designated the American Section of the International Association for Testing Materials. During this period, twenty-eight (28) Bulletins were issued. The Proceedings will appear hereafter in the form of annual volumes. The present volume, the first issued under the new plan of publication, is designated Volume II, the bulletins previously issued constituting Volume I. An abridged Table of Contents of Bulletins Nos. 1–28 follows:

*Bulletin No.* 1. *Issued April*, 1899. Pp. 1–8.
Minutes of the Organization Meeting, June 16, 1898.
Minutes of the Executive Committee, June 25, 1898, to February 22, 1899.
Minutes of First Annual Meeting, August 27, 1898.

*Bulletin No.* 2. *Issued July*, 1899. Pp. 9–12.
Provisional Program for the Second Annual Meeting.

*Bulletin No.* 3. *Issued August*, 1899. Pp. 13–16.
Officers of the American Section.
Program of the Second Annual Meeting.

*Bulletin No.* 4. *Issued September*, 1899. Pp. 17–26.
The Work of the International Association for Testing Materials. Annual Address by the Chairman, Mansfield Merriman.

*Bulletin No.* 5. *Issued October*, 1899. Pp. 27–52.
Preliminary Report on the Present State of Knowledge Concerning Impact Tests, by W. Kendrick Hatt and Edgar Marburg.

*Bulletin No.* 6. *Issued November*, 1899. Pp. 53–72.
Report of Second Annual Meeting, August 15–16, 1899.
Minutes of the Executive Committee to August 16, 1899.

*Bulletin No.* 7. *Issued January*, 1900. Pp. 73–80.
Minutes of the Executive Committee to January 6, 1900.
Miscellaneous Announcements.

*Bulletin No.* 8. *Issued May*, 1900. Pp. 81–86.
Proposed Standard Specifications for Structural Steel for Bridges and Ships.

*Bulletin No.* 9. *Issued May*, 1900. Pp. 87–92.
Proposed Standard Specifications for Structural Steel for Buildings.

*Bulletin No.* 10. *Issued May*, 1900. Pp. 93–100.
Proposed Standard Specifications for Open-Hearth Boiler Plate and Rivet Steel.

*Bulletin No.* 11. *Issued May*, 1900. Pp. 101–106.
Proposed Standard Specifications for Steel Rails.

*Bulletin No.* 12. *Issued May*, 1900. Pp. 107–110.
Proposed Standard Specifications for Steel Splice Bars.

*Bulletin No.* 13. *Issued May*, 1900. Pp. 111–114.
Proposed Standard Specifications for Steel Axles.

*Bulletin No.* 14. *Issued May*, 1900. Pp. 115–118.
Proposed Standard Specifications for Steel Tires.

*Bulletin No.* 15. *Issued May*, 1900. Pp. 119–124.
Proposed Standard Specifications for Steel Forgings.

*Bulletin No.* 16. *Issued May*, 1900. Pp. 125–128.
Proposed Standard Specifications for Steel Castings.

*Bulletin No.* 17. *Issued May*, 1900. Pp. 129–134.
Proposed Standard Specifications for Wrought Iron.

*Bulletin No.* 18. *Issued May*, 1900. Pp. 135–144.
Report of the American Branch of International Committee No. 1.

*Bulletin No.* 19. *Issued September*, 1900. Pp. 145–172
Program of the Third Annual Meeting.
Minutes of the Executive Committee, April 7, 1900.
Correspondence Relating to the Representation of the American Section on the International Council.

*Bulletin No.* 20. *Issued October*, 1900. Pp. 173–184.
Progress Report of the American Branch of International Committee No. 1.

*Bulletin No.* 21. *Issued March*, 1901. Pp. 185–214.
Announcement of International Congress of 1901.
Report of Third Annual Meeting, October 25–27, 1900.
Minutes of the Executive Committee to January 5, 1901.
Officers of the American Section for 1900–02.

*Bulletin No.* 22. *Issued May*, 1901. Pp. 215–216.
Program of the Fourth Annual Meeting.

*Bulletin No.* 23. *Issued June*, 1901. Pp. 217–230.
List of Members of the American Section.
By-Laws of the American Section.

*Bulletin No.* 24. *Issued June*, 1901. Pp. 231–236.
Revised Standard Specifications for Wrought Iron.

*Bulletin No.* 25. *Issued June*, 1901. Pp. 237–244.
Report of the American Branch of International Committee No. 1.

*Bulletin No* 26. *Issued July*, 1901. Pp. 245–246.
Letter Ballot on Proposed Standard Specifications.

*Bulletin No.* 27. *Issued August*, 1901. Pp. 247–262.
Report of Fourth Annual Meeting, June 29, 1901.

*Bulletin No.* 28. *Issued May*, 1902. Pp. 263–266.
Program of the Fifth Annual Meeting.

## PRICE LIST.

Bulletins Nos. 1, 2 and 3 are out of print. The remaining numbers may be obtained at the uniform price of ten cents (.10) postage prepaid, on application to the Secretary.

# OFFICERS

OF THE

# INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

PRESIDENT:

L. VON TETMAJER.

VICE-PRESIDENTS:

A. MARTENS, N. BELELUBSKI.

## MEMBERS OF COUNCIL:

Every country having a membership of twenty or more in the Association is entitled to a representative on the Council.

Austria—F. BERGER.
Belgium—A. GREINER.
Denmark—J. HANNOVER.
England—W. C. UNWIN.
France—H. LE CHATELIER.
Germany—A. MARTENS.
Holland—L. BIENFAIT.
Hungary—C. VON BANOVITS.
Italy—J. BENETTI.
Norway—S. A. LUND.
Roumania—A. O. SALIGNY.
Russia—N. BELELUBSKI.
Spain—M. CANO Y DE LEON.
Sweden—A. WAHLBERG.
Switzerland—F. SCHUELE.
United States of America—CHARLES B. DUDLEY.

SECRETARY:

ERNEST REITLER,

Nordbahnstrasse 50, Vienna, II, Austria.

Communications for the International Association should be directed to the President, Professor L. von Tetmajer, Imperial Technical High School, Karlsplatz, Vienna, IV, Austria.

## INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

### BY-LAWS.

Adopted at the Buda-Pesth Congress, 1901.

§ 1.

The Association shall be called "The International Association for Testing Materials."

§ 2.

The objects of the Association are: the development and unification of standard methods of testing; the investigation of the technically important properties of the materials of construction and other materials of technical importance, and also the perfecting of apparatus used for this purpose.

These objects will be furthered:

1. By the Congresses and other meetings of the Association.
2. By the publication of an official Journal.
3. By any other means that may appear desirable.

§ 3.

The funds necessary for carrying out the purposes mentioned in § 2 will be raised by

1. The annual subscriptions of members.
2. Profits from the official Journal.
3. Other contributions.

§ 4.

Any person may become a member upon being proposed by two members of the Association.

Official bodies and technical societies can be elected directly on their sending in their application for membership.

Applications for membership must be sent in writing to the President or to a member of the Council.

Resignations of membership must be sent in the same way.

§ 5.

It is the duty of every member to further the interests of the Society to the best of his ability.

Every member is required to pay an annual subscription of at least 6 Mks. = 6 shillings = $1.50.[1]

The Council is authorized to increase the annual subscription in order to cover extraordinary expenses incurred in the interests of the Association.

§ 6.

Every member has the right to obtain the Journal of the Association, during the period for which his subscription has been paid, on paying the fixed reduced price.[2]

§ 7.

The Association will hold a Congress, as a rule, every second year.

The arrangements for the Congresses will be discussed at general meetings and in meetings of the different sections.

Sections will be formed for the different groups of materials as may be considered necessary.

At present there are three sections:

I. Metals.

II. Natural and artificial building stones, cements and mortars.

III. Other materials of technical value.

Any special questions relating to the subjects of the different sections will be considered at sectional meetings.

The members assisting at the sectional debates, under the presidency of a member of the Council, will appoint the governing bodies of the different sections.

The results of the deliberations of the different sections must be communicated at a general meeting which will pass resolutions embodying the proposals of the sections.

[1] Subscriptions are to be paid to the duly appointed collectors in each country, the card of membership serving as a receipt. Subscriptions not paid by the first of July are collected through the post office.

[2] The reduced price has been fixed at 10 Mks. = ten shillings = $2.50. This sum may be sent in with the subscriptions. The yearly volumes begin on January 1.

Reports of Commissions, proposals of the Council and other matters to be laid before the Congress, will be printed in German, French and English, and will be sent (in the language preferred) to all members who have announced their intention of taking part in the Congress, within fourteen days before the meeting of the Congress, if possible.

The decisions of the Congress will be printed in all three languages and sent to all members of the Association.

§ 8.

The Council of the Association will transact all necessary business connected with the Association.

The Council will consist of the President and the duly elected members.

Every country represented in the Association by at least twenty members has the right to propose one member as member of the Council.

The President will be elected by the Congress, the Council by the members belonging to the different countries.

Till such election has taken place the former members of the Council remain in office.

The names of proposed new members of the Council have to be communicated to the President before each Congress.

The two Vice-Presidents will be elected by the Council from among its own members.

The Council is entitled to transact business when it has been duly called together according to rule and when the President or one of the Vice-Presidents is present.

Members of the Council may be re-elected.

If a member of the Council resigns during his term of office, the President shall immediately direct the election of a substitute by the members belonging to the country in question.

In the event of the death or resignation of the President, the Council will appoint one of its members to carry on the presidential duties till the next Congress.

The term of office of the Council lasts from one Congress till the next.

§ 9.

The business of the Association will be attended to by the President, assisted by a paid Secretary.

The members of the Council will attend to the business of the Association in the country which they represent.

§ 10.

The resolutions of the Congresses on technical questions merely serve to express the opinion of the majority. They are therefore in the form of recommendations and are in no way binding.

§ 11.

The resolutions of the Congresses can only be carried if at least three-fourths of the recorded votes are in favor of them. Every member of the Association present, as well as every representative of official bodies and technical societies has one vote.

The rights and duties of a member of the Association are not altered by the fact of his belonging at the same time to a national or other Association which Association is itself a member of the International Association.

§ 12.

The technical problems to be considered by the Association will be decided upon by the Congresses and by the Council and will be duly referred to commissions or reporters appointed by the Council.

§ 13.

The Council draws up its own regulations according to the By-Laws of the Association and to the needs which may from time to time present themselves.

§ 14.

In the event of the Association being dissolved, any funds belonging to it will be handed over to the "International Red Cross Association."

## THE INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

TECHNICAL PROBLEMS, COMMITTEES* AND REFEREES.

As constituted on October 20, 1902.

### SECTION A.

METALS.

**Problem 1.**—On the basis of existing specifications, to seek methods and means for the introduction of international specifications for testing and inspecting iron and steel of all kinds. (Proposed at the Zurich Congress 1895.)

*Committee:*

*Chairman*, A. Rieppel Aeussere Cramer-Klettstrasse 12, Nuremburg, Germany.

*Vice Chairman*, G. Alpherts, Koninginnegracht 66, Hague, Holland.

*American Members*, H. H. Campbell, James Christie, Carnegie Steel Company, represented by John McLeod; Franklin Institute, represented by Wm. H. Wahl, Paul Kreuzpointner, R. G. Moldenke, W. R. Webster, Walter Wood.

**Problem 2.**—To establish methods of inspection and testing for determining the uniformity of individual shipments of iron and steel. (Proposed at the Stockholm Congress 1897.)

*Committee:*

*Chairman*, W. Ast, Nordbahnhof, Vienna, Austria.

*Vice Chairman*, (office vacant).

*American Members*, Booth, Garrett and Blair, Thos. Gray, Gus. C. Henning, Paul Kreuzpointner, A. A. Stevenson, W. R. Webster, Albert Sauveur.

*The names of only the Chairmen, the Vice Chairmen, and American Members of International Committees are here given.

**Problem 3.**—On the properties of soft steel (Flusseisen) at abnormally low temperatures. (Proposed at the Zurich Congress 1895.)
*Referee*, (office vacant).

**Problem 4.**—Methods for testing welds and weldability. (Proposed at the Zurich Congress 1895.)
*Referee*, R. Krohn, Gutehoffnungshuette, Sterkrade, Germany.

**Problem 5.**—Collection of data for establishing standard rules for piece tests, with special reference to axles, tires, springs, pipes, etc. (Proposed at the Zurich Congress 1895.)

*Committee:*

*Chairman*, W. Rayl, Nordbahnstrasse 50, Vienna II, Austria.
*Vice Chairman*, A. Sailler, Favoritenstrasse 20, Vienna IV, Austria.
*American Members*, M. H. Wickhorst, H. V. Wille.

**Problem 6.**—On the most practical methods of polishing and etching for the macroscopic study of iron and steel. (Proposed at the Zurich Congress 1895).
*Referee*, E. Heyn, Carmerstrasse, 15, Charlottenburg, Germany.

**Problem 25.**—To establish uniform methods of testing cast iron and finished castings. (Proposed at the Buda-Pesth Congress 1901.)

*Committee:*

*Chairman*, C. Juengst, Kurfuerstendamm, 214 p. Charlottenburg, Germany.
*Vice Chairman*, R. G. Moldenke, P. O. Box 432, New York, N. Y.
*American Members*, Alex. E. Outerbridge, Jr., Albert Sauveur, Thos. D. West.

**Problem 26.**—Tests with notched bars for ascertaining the relations between the different methods of testing and for fixing the numerical values representing the different properties of metals. (Proposed at the Buda-Pesth Congress 1901).
*Referee*, Ed. Sauvage, Rue Eugène Flachat 14, Paris, France.

**Problem 27.**—Ball-pressure tests for ascertaining the relations between the different methods of testing and for fixing the

numerical values representing the different properties of metals. (Proposed at the Buda-Pesth Congress 1901.)
*Referee*, A. Wahlberg, Techn. Hochschule, Stockholm, Sweden.

**Problem 28.**—The consideration of the magnetic and electric properties of materials in connection with their mechanical testing. (Proposed at the Buda-Pesth Congress 1901.)
*Referees*, K. Hochenegg, Techn. Hochschule, Karlsplatz, Vienna, IV, Austria; M. von Hoor Tempik, Kgl. techn. Hochschule, Buda-Pesth, Hungary.

## SECTION B.

### NATURAL AND ARTIFICIAL BUILDING STONES AND THEIR CEMENTS.

**Problem 7.**—On the weathering qualities of roofing slates. (Proposed at the Zurich Congress 1895.)

*Committee:*

*Chairman*, A. Hanisch, Schellinggasse 13, Vienna I, Austria.
*Vice Chairman*, P. Larivière, Quai Jemmapes 170, Paris, France.
*American Members*, J. F. Kemp, Mansfield Merriman.

**Problem 9.**—On rapid methods for determining the strength of hydraulic cements. (Proposed at the Zurich Congress 1895.)

*Committee:*

*Chairman*, F. Berger, Rathhaus, Vienna 1, Austria.
*Vice Chairman*, L. von Tetmajer, Techn. Hochschule, Karlsplatz, Vienna IV, Austria.
*American Members*, W. W. Maclay, Chas. F. McKenna.

**Problem 10.**—To digest and evaluate the resolutions of the conferences of 1884–1893 concerning the adhesive qualities of hydraulic cements.
*Referee*, J. Maluga, Newsky 182, St. Petersburg, Russia.

**Problem 11.**—To establish methods for testing puzzolanas with the object of determining their value for mortars. (Proposed at the Zurich Congress 1895.)

*Committee:*

*Chairman*, G. Herfeld, Andernach, Germany.
*Vice Chairman*, C. Segré, Ancona, Italy.
*American Member*, A. Lundteigen.

**Problem 12.**—Investigation on the behavior of cements as to time of setting and on the best method for determining the beginning and the duration of the process of setting. Proposed at the Zurich Congress 1895, enlarged in conformity with the resolution of the Buda-Pesth Congress 1901.)

*Committee:*

*Chairman*, E. Candlot, rue d'Edimbourg 18, Paris, France.
*Vice Chairman*, N. Lamine, Zabalkansky 9, St. Petersburg, Russia.
*American Members*, Spencer B. Newberry, Clifford Richardson.

**Problem 13.**—On the normal consistency of cement mortars for test specimens. (Proposed at the Zurich Congress 1895.)

*Committee:*

*Chairman*, A. Greil, Rathhaus, Vienna I, Austria.
*Vice Chairman*, L. von Tetmajer, Techn. Hochschule, Karlsplatz, Vienna IV, Austria.
*American Member*, R. L. Humphrey.

**Problem 14.**—On the influence of sulphurous acid on artificial stones and mortars. (Proposed at the Stockholm Congress 1897.)
*Referee*, Dr. Karl Goslich, Zuellchow, Pomerania, Germany.

**Problem 15.**—On the action of sulphuretted hydrogen dissolved in water or in a gaseous condition upon different kinds of mortar. (Proposed at the Stockholm Congress 1897.) (To be treated as Problem 14, and eventually added to it.)
*Referee*, Dr. Karl Goslich, Zuellchow, Pomerania, Germany.

**Problem 29.**—Determination of the liter weight of cement. The strength of neat hydraulic cements. Determination of a standard sand. (Proposed at the Buda-Pesth Congress 1901.)
*Referees*, N. Belelubski, Rue Serpuchowskaja 4, St. Petersburg, Russia; F. Schuele, Eidg. Polytechnikum, Zurich, Switzerland.

**Problem 30.**—Determination of the simplest method for the separation of the finest particles in Portland cement by liquid and air processes. (Proposed at the Buda-Pesth Congress 1901.)

*Referee*, M. Gary, Kgl. mech.-techn. Versuchsanstalt, Charlottenburg, Germany.

**Problem 31.**—On the behavior of cements in sea water. (Proposed at the Buda-Pesth Congress 1901.)

*Referees*, H. Le Chatelier, Place du College de France 9, Paris, France; Prof. A. R. Choulatschenko, Saperny per 16, St. Petersburg, Russia.

**Problem 32.**—On accelerated tests of the constancy of volume of cements. (Proposed at the Zurich Congress 1895.)

*Committee:*

*Chairman*, Bertram Blount, Broadway Westminster, London, S. W., England.

*Vice Chairman*, (office vacant).

*American Members*, R. W. Lesley, Spencer B. Newberry.

**Problem 33.**—On the influence of the proportion of water and sand on the strength of Roman and other cements. (Proposed at the Buda-Pesth Congress 1901.)

*Referee*, The Hungarian Society for Testing Materials, Buda-Pesth, Hungary.

## SECTION C.

### OTHER MATERIALS.

**Problem 17.**—On methods of testing tile pipes. (Proposed at the Stockholm Congress 1897.)

*Referee*, M. Gary, Kgl. mech.-techn. Versuchsanstalt, Charlottenburg, Germany.

**Problem 18.**—On the methods of testing the protective power of paints used on metallic structures. (Proposed at the Zurich Congress 1895.)

*Referees*, Albert Grittner, Köbanyai út 30, Buda-Pesth, Hungary; E. Ebert, Centralbahnhof, Munich, Germany.

**Problem 19.**—On uniform methods for testing lubricants. (Proposed at the Zurich Congress 1895.)
*Referee*, N. Petroff, Zagorodny 70, St. Petersburg, Russia.

**Problem 23.**—On uniform methods for compression tests of wood.

*Committee:*

*Chairman*, Prof. A. Schwappach, Eberswalde, Germany.
*Vice Chairman*, A. Wykander, Goeteporg Sweden.
*American Member*, Filibert Roth.

**Problem 35.**—Study of the methods of testing caoutchouc. (Proposed at the Buda-Pesth Congress 1901.)

*Committee:*

*Chairman*, E. Camerman, Rue Philippe Le Bon 73, Brussels, Belgium.
*Vice Chairman*, (office vacant).
*American Member*, R. G. Pearson.

## SECTION D.

### MISCELLANEOUS SUBJECTS.

**Problem 22.**—Considering that the resolutions formed by the International Conferences of Munich, Dresden, Berlin, Vienna and Zurich for the purpose of attaining unity in the methods of testing materials and the report of the Committee of the American Society of Mechanical Engineers do not agree in many points with the decisions arrived at by the French commission, it is proposed: that the Council appoint a commission which shall prepare a report upon these differences, and proposals for ways and means of abolishing them.

*Committee:*

*Chairman*, N. Belelubsky, Rue Serpouchowskaya 4, St. Petersburg, Russia.
*Vice Chairmen*, A. Martens, Kgl. mech.-techn. Versuchsanstalt, Charlottenburg, Germany; E. Sauvage. l'Ecole des mines, Paris, France.
*American Members*, Albert Ladd Colby, Gus. C. Henning, R. G. Moldenke, George F. Swain, George S. Webster, W. R. Webster, Walter Wood.

**Problem 24.**—On uniform nomenclature of iron and steel. (Resolution of Council, February 3, 1901.)

*Committee:*

*Chairman*, H. M. Howe, 27 West Seventy-third street. New York, N. Y.

*Vice Chairman*, L. Lévy, Rue de La Rochefoucauld 19, Paris, France.

*American Member*, H. H. Campbell.

**Problem 34.**—Fixing a uniform definition and nomenclature of the bitumens. (Proposed at the Buda-Pesth Congress 1901.)

*Committee:*

*Chairman*, G. Lunge, Eidg. Polytechnikum, Zurich, Switzerland.

*Vice Chairman*, Jenoe Kovács, Tataros (Post Mezoe Telegd), Hungary.

*American Members*, A. W. Dow, Clifford Richardson.

## TECHNOLEXICON.

NOTE —The Executive Committee has directed that space be given to the following communication, both as a matter of general interest to the members of the Society, and with a view of inviting their collaboration as individuals in an enterprise of the greatest prospective usefulness to engineers and technical men in general.

The SOCIETY OF GERMAN ENGINEERS, with headquarters at Berlin, has begun the compilation of a universal *technical dictionary* for translation purposes, which is to be called the TECHNOLEXICON. It is to be in three parts, viz: volume I: German-English-French, volume II: English-German-French, volume III: French-German-English.

The want of a comprehensive work of this kind has been felt for a long time, and the enormous development of international competition in all technical and industrial branches has made it a matter of absolute necessity that technical and business men should be able at least to read several modern languages. For this purpose a thoroughly reliable dictionary is of prodigious importance, and this the Society intends to supply. The leading engineering societies of the countries interested have given their unqualified approval of the plan and are collaborating in the Technolexicon. A work of such vast scope is necessarily attended by vast difficulties. To convey some idea of these it need only be mentioned that a list of about 5000 specialties and sub-specialties has been drawn up, all of which will be represented in the new dictionary. The great mass of material involved may be readily imagined. It would be impossible for a staff of editors to carry this work through unaided. The Society has, therefore, issued circulars—to be had on application to the office, Dorotheenstr. 49, Berlin (N.W. 7) soliciting the coöperation of technical societies, colleges, industrial establishments, individuals competent to assist, etc., and also giving details concerning the different ways in which this coöperation can be effected.

The collaboration is expected to be rendered gratuitously as a rule. The Society has, however, engaged an international staff

of editors, under the leadership of the well-known lexicographer, Dr. HUBERT JANSEN, for the sifting, arranging and final molding of the material, and it will bear all the expenses of publication.

The price will be fixed as low as possible, even if the Society should lose thereby, as the Technolexic on is not a speculation but a work prompted by the wish to promote international intercourse.

The appeal has been responded to generously in all countries concerned; societies have taken the matter up and named collaborators, great firms have placed their catalogues, price-lists, etc., at the disposal of the editors for excerption, and many of them are collecting the words and expressions of their specialties. More than 1700 individual collaborators are doing all they can to further the work, and the hope is justified that the result will be as intended—a reliable technical dictionary, on a scale never before conceived.

To make this hope a certainty, however, many more collaborators are needed, for the specialties represented as well as for those that have not yet been taken up. That this work of general interest and usefulness should receive contributions from all sides is much to be desired, and the assistance of individual collaborators is most urgently requested by the Society, and will receive lasting recognition.

# VOLUME II.

## SUBJECT INDEX.


**American Society for Testing Materials.**

Annual Report of the Executive Committee of the —, 358.
By-Laws of the —, 322.
Charter of the —, 320.
Contents of Previous Publications of the —, 366.
Deceased Members of the —, 356.
General Information Concerning the —, 328.
Geographical Distribution of Members of the —, 356.
List of Members of the —, 332.
Officers, Members of the Executive Committee, and Standing Committees of the —, 331.
Past Officers of the —, 357.
Report of Auditing Committee of the —, 365.

**Address.**

Annual — by the Retiring President, Henry M. Howe, 17.

**Beams.**

Tests of Reinforced Concrete —. W. Kendrick Hatt, 161.

**Bridges.**

Is it Desirable to Specify a Single Grade of Structural Steel for — of Ordinary Spans? Topical Discussion, 50.

**Cast Iron.**

A Quick and Automatic Taper-Scale Test for —. Asa W. Whitney, 217.
Effect of Variations in the Constituents of —. W. G. Scott, 181. Discussion, 200.
High Strength of White Iron Castings as Influenced by Heat Treatment. Alex. E. Outerbridge, Jr., 229. Discussion, 240.
Notes on Current Specifications for — Pipe. Walter Wood, 243.
On the Constitution of —. Henry M. Howe, 246. Discussion, 276.
Present Status of Testing —. Richard Moldenke, 207. Discussion, 213.
The Need of Foundry Experience for the Proper Inspection and Testing of —. Thos. D. West, 210. Discussion, 213.

**Cement.**

— Testing in Municipal Laboratories. Richard L. Humphrey, 150.
Standard — Specifications. R. W. Lesley, 121. Discussion, 133.
The Advantages of Uniformity in Specifications for — and Methods of Testing. George S. Webster, 128. Discussion, 133.
The Chemical Analysis of —: Its Possibilities and Limitations. Richard K. Meade, 139. Discussion, 146.

**Concrete.**
Tests of Reinforced — Beams. W. Kendrick Hatt, 161.
**Impact Tests.**
See **Tests.**
**International Association for Testing Materials.**
By-Laws of the —, 370.
General Information Concerning the —, 326.
Officers of the —, 369.
Report on the Buda-Pesth Congress of the —. Henry M. Howe, 279.
Technical Problems, Committees and Referees of the —, 374.
**Laboratories.**
Cement Testing in Municipal —. Richard L. Humphrey, 150.
**Pipe.**
Notes on Current Specifications for Cast Iron —. Walter Wood, 243.
**President's Address.**
See **Address.**
**Proceedings.**
— of the Fifth Annual Meeting, 17.
Summary of —, Fifth Annual Meeting, 5.
**Rails.**
Proposed Modifications of the Standard Specifications for Steel —. Topical Discussion, 23.
Finishing Temperature and Structure of Steel —. Albert Sauveur, 79. Discussion, 85.
— Temperatures. Simon Strock Martin, 75. Discussion, 85.
**Reinforced Concrete.**
See **Concrete.**
**Rivets.**
Steel —. Gaetano Lanza, 110.
**Specifications.**
Is it Desirable to Specify a Single Grade of Structural Steel for Bridges of Ordinary Spans? Topical Discussion, 50.
Notes on Current — for Cast Iron Pipe. Walter Wood, 243.
Proposed Modifications of the Standard — for Steel Rails. Topical Discussion, 23.
Standard Cement —. R. W. Lesley, 121. Discussion, 133.
The Advantages of Uniformity in — for Cement and Methods of Testing. George S. Webster, 128. Discussion, 133.
**Standard Specifications.**
See **Specifications.**
**Steel.**
Finishing Temperature and Structure of — Rails. Albert Sauveur, 79. Discussion, 85.
Is it Desirable to Specify a Single Grade of Structural — for Bridges of Ordinary Spans? Topical Discussion, 50.

Proposed Modifications of the Standard Specifications for — Rails. Topical Discussion, 23.
— Rail Temperatures. Simon Strock Martin, 75. Discussion, 85.
— Rivets. Gaetano Lanza, 110.
The Relation between the Basic Open-Hearth Process and the Physical Properties of —. Topical Discussion, 97.

**Technolexicon.**

Technolexicon, 381.

**Testing.**

See also **Tests.**
Bibliography on Impact Tests and Impact — Machines. W. Kendrick Hatt and Edgar Marburg, 283.
Cement — in Municipal Laboratories. Richard L. Humphrey, 150.
Present Status of — Cast Iron. Richard Moldenke, 207. Discussion, 213.
The Advantages of Uniformity in Specifications for Cement and Methods of —. George S. Webster, 128. Discussion, 133.
The Ethics of —. Paul Kreuzpointner, 118.
The Need of Foundry Experience for the Proper Inspection and — of Cast Iron. Thos. D. West, 210. Discussion, 213.

**Testing Laboratories.**

See **Laboratories.**

**Testing Machines.**

Bibliography on Impact Tests and Impact —. W. Kendrick Hatt and Edgar Marburg, 283.

**Tests.**

Bibliography on Impact — and Impact Testing Machines. W. Kendrick Hatt and Edgar Marburg, 283.
Quick and Automatic Taper-Scale —. Asa W. Whitney, 217.
Rules for Standard — of Materials Formulated by the German Association for Testing Materials (English Translation), 298.
— of Reinforced Concrete Beams. W. Kendrick Hatt, 161.

## AUTHOR INDEX.

**Aiken, W. A.**
Discussion, 133.
**Boller, A. P.**
Is it Desirable to Specify a Single Grade of Structural Steel for Bridges of Ordinary Spans? 50.
**Campbell, H. H.**
Discussion, 24, 26, 27, 31, 39, 44, 46, 70, 74, 94, 99, 102.
**Carhart, P. E.**
Discussion, 29, 34, 96.
**Christie, James.**
Discussion, 42, 73.
**Condron, T. L.**
Is it Desirable to Specify a Single Grade of Structural Steel for Bridges of Ordinary Spans? 52.
Discussion, 69, 72, 102.
**Cooper, Theodore.**
Is it Desirable to Specify a Single Grade of Structural Steel for Bridges of Ordinary Spans? 57.
**Dudley, Charles B.**
Discussion, 23, 40, 42, 49, 74, 91, 92, 93, 97, 101, 102, 106, 201, 215, 216, 276.
**Dudley, P. H.**
Discussion, 85.
**Flagg, Stanley G., Jr.**
Discussion, 241.
**Greiner, J. E.**
Is it Desirable to Specify a Single Grade of Structural Steel for Bridges of Ordinary Spans? 58.
Discussion, 68.
**Hatt, W. Kendrick.**
Tests of Reinforced Concrete Beams, 161.
Bibliography on Impact Tests and Impact Testing Machines, 283.
**Howe, Henry M.**
Annual Address by the Retiring President, 17.
On the Constitution of Cast Iron, 246.
Report on the Buda-Pesth Congress, 279.
Discussion, 27, 34, 37, 38, 47, 90, 278.
**Humphrey, Richard L.**
Cement Testing in Municipal Laboratories, 150.
Discussion, 135.
**Huston, Charles L.**
Discussion, 49.

**Job, Robert.**
Discussion, 25, 26, 27, 31, 33, 37, 38, 39, 89, 135.
**Kenney, E. F.**
Discussion, 30, 37, 93, 95.
**Kreuzpointner, Paul.**
The Ethics of Testing, 118.
Discussion, 39, 103, 201.
**Lanza, Gaetano.**
Steel Rivets, 110.
**Lesley, R. W.**
Standard Cement Specifications, 121.
Discussion, 135.
**McLeod, John.**
Is it Desirable to Specify a Single Grade of Structural Steel for Bridges of Ordinary Spans, 60.
Discussion, 23, 39, 41, 42, 43, 69, 91, 93, 94, 95.
**Marburg, Edgar.**
Bibliography on Impact Tests and Impact Testing Machines, 283.
**Martin, Simon Strock.**
Rail Temperatures, 75.
**Meade, Richard K.**
The Chemical Analysis of Cement: Its Possibilities and Limitations, 139.
**Moldenke, Richard.**
Present Status of Testing Cast Iron, 207.
Discussion, 200, 205, 215, 240, 241, 278.
**Outerbridge, Alex. E., Jr.**
High Strength of White Iron Castings as Influenced by Heat Treatment, 229.
Discussion, 214, 241.
**Sauveur, Albert.**
Finishing Temperature and Structure of Steel Rails, 79.
Discussion, 27, 37, 45, 96, 204, 205, 213, 214, 216, 240, 276.
**Schneider, C. C.**
Is it Desirable to Specify a Single Grade of Structural Steel for Bridges of Ordinary Spans? 62.
**Scott, W. G.**
Effect of Variations in the Constituents of Cast Iron, 181.
**Snow, J. P.**
Is it Desirable to Specify a Single Grade of Structural Steel for Bridges of Ordinary Spans? 66.
Discussion, 73.
**Taylor, W. P.**
Discussion, 134.
**Thackray, George E.**
Discussion, 29, 34, 71, 95.

**Voorhees, S. S.**
Discussion, 32, 204, 205.
**Webster, George S.**
The Advantages of Uniformity in Specifications for Cement and Methods of Testing, 128.
**Webster, William R.**
Discussion, 27, 43, 45, 49, 69, 74, 96, 104.
**West, Thos. D.**
The Need of Foundry Experience for the Proper Inspection and Testing of Cast Iron, 210.
Discussion, 203, 204, 213, 215.
**Whitney, Asa W.**
A Quick and Automatic Taper-Scale Test, 217.
Discussion, 200, 202, 203, 205.
**Wille, H. V.**
Discussion, 107
**Wood, Walter.**
Notes on Current Specifications for Cast Iron Pipe, 243.